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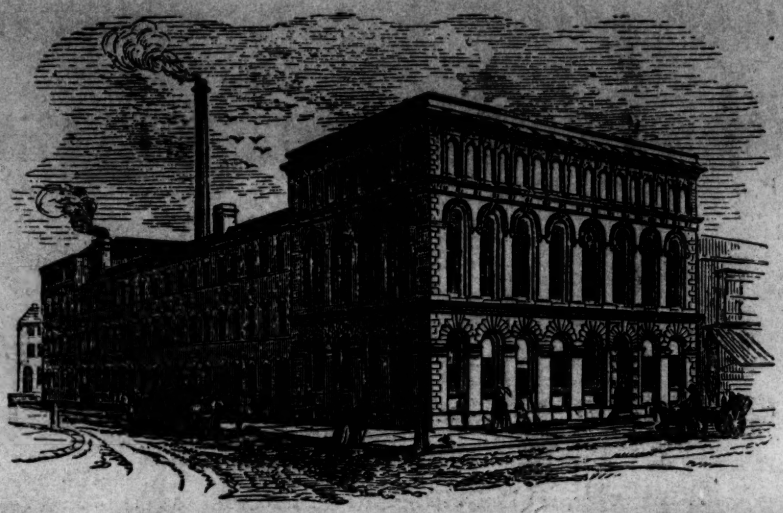

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PRACTICAL NOTES ON "HETEROGENESIS,"

A REPUTED FEATURE OF SPONTANEOUS GENERATION.

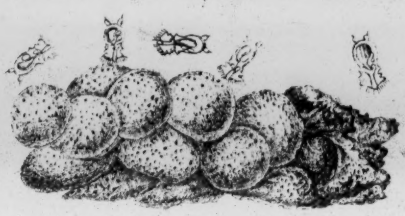
By the Rev. W. H. DALLINGER, V.P.R.M.S.

[PLATES CXXXIX. AND CXL.]

THE doctrine of "spontaneous generation" is declared by its principal advocate* to involve not merely the origin of living forms from not-living elements, but also the origination from living beings, more or less complex in organisation, of other living units wholly different from themselves, and having no tendency to assume or revert to the parental type. This means briefly that one organised form may, by the operation of some occult laws, produce another organised form wholly unlike itself, and which may be not only of a different genus, but of a different order of a different class—nay, probably altogether of a different kingdom. This is an assertion of caprice in biological laws. Their action is uncertain. True, these remarkable phenomena are not at present asserted of the more easily accessible and highly developed organisms; but it may be instructive to note that if they were so applicable it would admit, for example, of a humming-bird being hatched from a snake's egg, or a gorilla being born from a kangaroo; for neither of these instances is more startling than the alleged transformation of Euglenæ and Chlorococcus directly into—rotifers! Yet not only this, but many other things equally as remarkable, are sanctioned and asserted by Dr. Bastian; and the credence of biologists is asked for these affirmations, with as little apology or hesitation, as for the fact that a tadpole is the precursor of a frog, or a chrysalis of a butterfly.

This of course gives much greater complexity to the hypothesis of spontaneous generation; but at the same time it gives it character. It is at least unique.

* "The Beginnings of Life." Dr. H. C. Bastian. Vol. i. p. 244.



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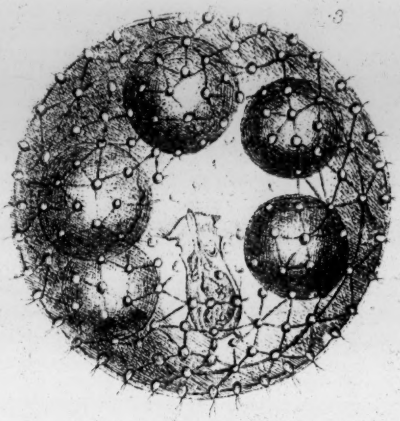

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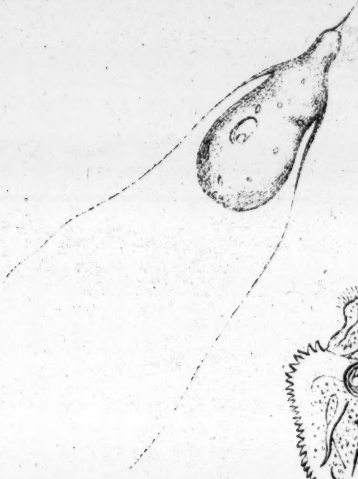

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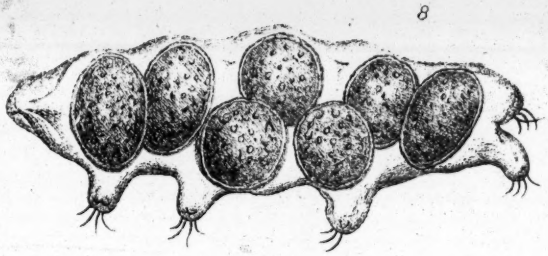

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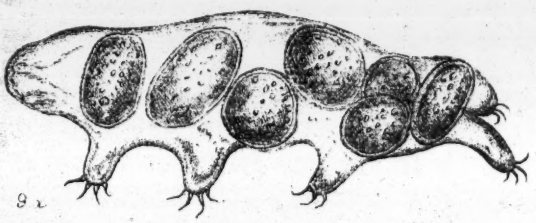

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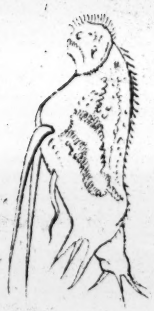

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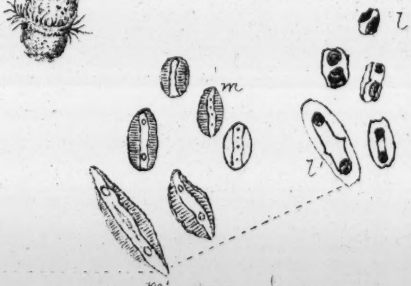

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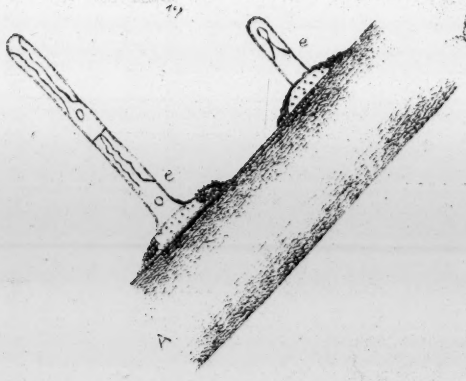

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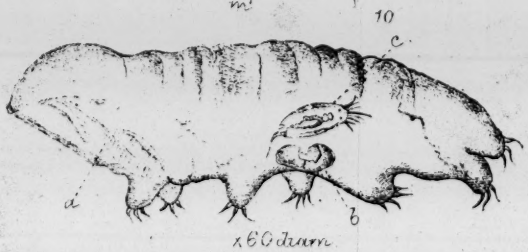

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Illustrations of mistaken "Heterogenesis"

The former part of the hypothesis—that living forms originate in not-living elements—arise, *de novo*, from dead matter—has received, and is still receiving, the most careful and persistent consideration of biologists; and the present balance of facts may safely be accepted from the pen of Professor Huxley, who, in a most comprehensive and elaborate article on "Biology," in the edition of "Encyclopædia Britannica," now passing through the press, affirms that "The biological sciences are sharply marked off from the abiological, or those which treat of the phenomena manifested by not-living matter, in so far as the properties of living matter distinguish it absolutely from all other kinds of things, and as the present state of knowledge furnishes us with no link between the living and the not-living." *

But the latter part of the hypothesis with which we are now dealing—that of the production of one kind of organism by another of an altogether different nature—although it has had some irresistible refutations, has not been seriously considered by the majority of leading biologists. The reason is plain. It is absolutely unsustained by facts. It is based on careless and incompetent observation, or exaggerated inference. It is out of harmony with all the most valuable observations of the most careful observers, and contradicts all we have otherwise learned of nature's methods. Its refutation, so far as it need be accomplished, may be safely relegated to specialists to be dealt with in detail, and this in more than one instance has been already done with remarkable effect. The philosophical biologist can afford to discard it; it does not require his serious consideration; his experience has taught him its fallacy; whilst its influence upon the student can neither be great nor lasting. To every mind, indeed, it should be apparent that, before such remarkable affirmations are presented as scientific *facts*, every pains should have been taken to make them such. These extraordinary "transformations," which are not only alleged, but plentifully figured, ought surely to have been so scrutinised, repeated, controlled, and purged of all fallacy, as to make the recorded phenomena at least as certain as the manner of recording them would lead the reader to infer it was, and as the seriousness of the issue demanded. But that this has not been the case is painfully apparent. In the instances of reputed "transformation" which are the result of Dr. Bastian's personal investigation, there is a looseness of method, and a disregard of detail, minutiæ, and above all, continuity of research, which stands in singular contrast to the precision and persistence of modern science, even in the simplest matters. But the surprise which this awakens is enhanced by the fact that

* Vol. iii. p. 679, 9th Ed. 1875.

he gives without question, nay, with implicit sanction, a series of "heterogenetic" phenomena from "the much neglected memoir of Dr. Gros" and others. The former has been for twenty years before the scientific world, during which time unparalleled vigour has been displayed by biologists in every department of research, and especially in the development of minute life-forms; which have been studied with constantly improving apparatus. But during the whole of this time not a single instance of accepted corroboration of these strange transformations can be pointed out. But thousands of observations have been made and recorded that are directly adverse to the whole. Surely this alone should have suggested caution, and the careful and competent repetition of both Gros' and his own observations. But it was not so.

All who have any practical knowledge of the nature of such enquiries will admit that an indispensable condition—a *sine quâ non*—to accurate results, is *continuity of observation*; and that, moreover, upon the same organisms, and with the very best appliances; all of which should be guarded by the elimination of every conceivable source of fallacy.

Discontinuous or interrupted observation is, in such inquiries, worse than useless. It is at once a prolific and a fascinating source of error. The same must be said of not working out the history of the *same individual*. In all such researches scores, nay, hundreds of hours are wasted—or rather apparently lost—in following the organism to a certain point, and then it dies; or some accident happens, or some distraction to the observer arises; and the temptation is to take up the observation again *upon another form in apparently the same developmental condition as the former one was in when the interruption happened*. No source of error can be more serious in practice, when the objects of research are so minute and unknown. Hundreds of instances of this might be given. Indeed there can be no accuracy unless the observation begins again *ab initio*, and is carried persistently to the end.

Let any one take a moderately decomposed infusion of fish or brain, and put a small clear drop of it from the point of a fine "dipper into half a wine-glass of Cohn's nutritive fluid, and leave it for a few hours. Let a drop of this be put on in the usual way upon the continuous moist stage described in the "Researches into the Life-History of the Monads," and let a $\frac{1}{16}$ in. objective be used. The probability is that for two days nothing will be visible to the most careful scrutiny but bacteria—at least it can be so arranged. But now, the observer who spends two or three hours discontinuously at the instrument will probably observe that what *look like* some of the bacteria are getting larger; and if then a night should intervene, he will,

on returning to his instrument in the morning, be startled by discovering that an anchored springing monad is dotted over the "field," with perhaps another form swimming freely about. Now in this case the inference would be more legitimate than in the majority of Dr. Bastian's that the bacteria had been "transformed" into monads. There were the bacteria, alone—some of them were seen to enlarge—and here in the course of a few hours are vigorous and distinctive monads! *Post hoc, propter hoc.* But let this group of monads be now casually examined in the same way: in the course of some hours many of these will become very still and sac-like; the crowding of the field will increase; the difficulty of discrimination to interrupted observation will become greater; and at the end of twelve hours more, some splendid specimens of *Kerona pustulata* will be asserting their mastery of the field. Now there is no instance presented by Dr. Bastian, as a case of "transformation," that has a more exact basis to stand upon than is presented here for inferring that the bacteria were transformed into monads, and that these monads preferred to perpetuate existence as Kerona. But what are the facts? The apparently fattening or enlarging bacteria were simply developing monad germs, and if *continuously* watched all the steps from the germ to the adult form would have been seen; and the same continuity of observation would have shown that the Kerona had a genetic origin as independent of the monads as the monads had of the bacteria. Indeed, low in the scale of organisation as the monads are, I do not hesitate to affirm that in their development they give no sanction or shadow of support to the hypothesis of "heterogenesis." "On the contrary, the life-cycle of a monad is as rigidly circumscribed within defined limits as that of a mollusc or a bird. There is no indication of any unusual or more intense method of specific mutation than those resulting from the secular processes involved in the Darwinian law, which is held to furnish the only legitimate theory of the origin of species." *

But to the young and ardent observer the example set by Dr. Bastian might prove the utmost evil. It is possible to infer almost anything from discontinuous observation. The simplest cases illustrate this. Only recently a friend, an ardent microscopist, announced to me his discovery of a perfect demonstration of "heterogenesis." We had some days before made an accidental gathering of the finest specimens of *Volvox globator* which I had ever seen. These were placed in a small clean trough, with clear water. With 100 diameters nothing of moment was visible but the volvoxes. The trough containing them was put

* "Further Researches into the Life-History of the Monads." Dallinger and Drysdale. 'M. M. J.' vol. xiii. p. 189.

into a moist chamber, to prevent evaporation, and left for four days. Quite by accident it was examined again, when to the surprise and extacy of my friend there was nothing living in the trough but rotifers. The volvoxes were all dead, and lay at the bottom of the trough, and the rotifers were voraciously eating them. More than that, in two of the dead volvoxes the form of a similar rotifer could be distinctly seen imbedded. My attention was called to the fact as a "transformation." I at once saw that this was a case, constantly re-appearing, of the rotifer parasitic within the volvox; and as I had never seen this, or the rotifer itself, before, I made a drawing of the whole field of dead volvoxes with rotifers devouring them, as seen with 12 diameters, and given at fig. 1, Pl. CXXXIX., and also made a careful drawing of the rotifer alone under 80 diameters, which is given in fig. 2. I remembered that Ehrenberg had observed at least two such rotifers, and on referring, found that my drawing came nearest to his *Notommata parasitica*. There are minute differences between the form I saw and Ehrenberg's, and there is a discrepancy in size; but there can be little doubt that it is either this form or a variety. I was now induced to make another gathering; and within the cells of four out of some thousands of volvoxes I saw the rotifer moving with sudden fierce jerks, devouring the young and eventually bursting the parent cell and escaping. A drawing of the volvox with its parasitic rotifer is seen in fig. 3, where the former is slightly out of focus that the latter might be clearly perceived.

Of course no practical naturalist would have been for a moment mistaken here. But a want of knowledge of all the facts, and a bias to a certain theory, could easily conclude that this was a case of volvoxes being "transmuted" into rotifers. And the same kind of inference is a possible danger to any observer with a bias, when the objects are comparatively inaccessible, or their life-histories unknown. But in the instance given, would it not be equally just to infer that the *Trichina spiralis* in a man or a pig was "transmuted" muscle? or that *Toenia solium* was—say the "transmuted" villi of the digestive tract?

But a case yet more instructive presents itself. In the June of 1874, a friend at that time residing at Sandhurst, in Berkshire, sent me a large bottle of water from a pond there, containing a remarkable monad, with two trailing flagella and a swiftly lashing anterior one. The form was quite new to me, and is seen in fig. 4. I did all in my power to keep it alive, that I might if possible work out its history when some work then claiming all my time should be finished. The water containing the monads was placed in circumstances that would as far as possible prevent evaporation and admit air. But they rapidly diminished in number until the middle of August, and

then remained stationary until the early part of September was past, and then there appeared suddenly, a minute, and to me entirely unknown rotifer. It was one of remarkable form and deportment, which I at once sketched, and have reproduced at fig. 5. I now examined with great care, and found the monad but feebly present; indeed only a very few were in the normal condition and shape, whilst a considerable number were slowly moving in the shape drawn at fig. 6. Now this was grotesque in the highest degree; for the newly imported rotifer had decidedly a portrait of its own when seen in profile, as fig. 7 will testify; and the modified monad approximated to this in a simply ludicrous manner. The absurd caricature seen in fig. 6 was enhanced by amœboid elongations and sharpenings of the lower part of the body, and by the protrusion of pseudopodal spines at *a*, *b*, and *c*; which still further pointed to the hypothesis that this simple creature, by virtue of the "laws" of "heterogenesis," was aiming at a higher sphere. Certainly, to a mind that could see its way to "heterogenesis," this was a suggestive instance, and might have been fairly employed (on the pattern already presented) to swell the instances of the "transmutation of monads into rotifers."

But a further acquaintance with the monad wholly dispels this dream: it was merely passing honestly through a phase in its life-history, after the fashion of its ancestors. And as to the rotifer, my attention was afterwards called to the fact that Mr. Gosse had seen and figured it; and in his account of it, I find a most instructive passage.* He names it *Dinocharis Collinsii*, and tells us that a bottle of the water in which this rotifer was found was taken away by a friend; but although it was well searched the rotifer was not found, nor indeed was "anything of interest" discovered. It was nevertheless retained; and after having been kept for more than four months it was suddenly and in an unexpected manner seen to be "swarming with these interesting creatures"—an event extremely similar to the one I record; only, in this latter case, there were no monads beforehand to suffer "transmutation."

Now whoever has carefully read the reputed facts for "heterogenesis" will be fain to admit that there are very few of them that offer more reasonable ground for the inference made than exists in this instance. Discontinuous observation, aided by imagination, sees the monad, then the form of a monad mid-way between itself and the rotifer, and finally the latter; and, after its fashion, the case would be established. That this is no exaggeration of the kind of reasoning employed, we may fairly test by the facts presented.

* "Intellectual Observer," vol. x. pp. 270-1.

Most microscopists have at some time made acquaintance with the water-bears of our ponds, and a good many have followed their development. Whoever has done the latter has fully convinced himself of the truth of the statements of Kölliker, Frey, Doyère, Kaufmann and others, that the tardigrades in every instance produce large fecundated eggs, from which young, closely resembling the parents, emerge. Another feature of the tardigrades is the extreme hardness and toughness of their "skin." It is in point of fact, speaking relatively to the Arthoropoda, almost a "shell." This skin it is also well known is "cast" by the creature, and it forms, in the case of the female, a shelter or protection for her eggs.

Now Dr. Bastian tells us that the power of reproduction in these forms is not limited to the "rudimentary generative organ," because "Dr. Gros tells us that the dead tardigrades may ultimately be resolved into specimens of Actinophrys, Peranemata, or Arcellinæ," and that these products may at different times be either all of one kind, or intermixed with each other and with young tardigrades! On the strength of this discovery we are presented with a drawing which I reproduce, fig. 8. The subscription which accompanies this is very suggestive. It runs thus, viz.: "Seven large germs *into which the total internal substance of the parent has become resolved*, each of them being *capable of developing into a tardigrade.*"

Now, wherever there are plenty of tardigrades there will be found dead forms, with their internal structure unchanged, and others which are mere empty shells or skins. Some of these latter are, doubtless, "cast skins;" but the dead water-bears, in a trough not very plentifully supplied with food, will soon be attacked by *paramecia*; and although the aperture they make may not be clearly seen, they somehow get *into* the body of the animals, and gradually devour all that is in it; and after cleaning it as thoroughly as ants will a small skeleton, leave it a hollow but perfect form. It is now open to the chapter of accidents, and it can be no matter for surprise that the minute eggs of aquatic creatures enter it and hatch there.

This may be easily illustrated. Mr. G. F. Chantrell, the Secretary of the Microscopical Society of Liverpool, is a very careful and constant observer of pond-life. He has endeavoured to verify or substantiate some of the more marked cases presented by Dr. Bastian. But his method of examination is, of necessity, an interrupted one. He has frequently called my attention to curious cases of apparent "transmutation;" and I have before me now some of his drawings of these, taken from nature. In fig. 9 I reproduce one, which it will be seen is extremely like the one figured by Bastian (fig. 8), which, it must be remembered, he affirms, on the assurance of Gros, was

full of germs by the *resolution* of its internal substance; and that each of the germs was "capable of developing into a tardigrade." But, fortunately, Mr. Chantrell did not leave the germs to their capabilities; he suspiciously followed them out, and they became *Stentor Caeruleus*! As drawn after hatching, they are presented in the attached or fixed state at 9*a*, and in the free swimming condition at 9*b*. Clearly the eggs of the stentor had got into the dead hollow body of the tardigrade, and developed there!

That this inference is a correct one I have repeatedly verified, and at fig. 10 give an additional instance in proof. This is the hollow, perfectly transparent skin of a tardigrade. Nothing has been left within but the hard retractile tube and "gizzard," and these, as seen at *a*, have fallen from their true position. At *b* a small oval body was seen, perfectly, and watched; and eventually the small rotifer *c*—probably *Monura dulcis*—emerged from it, and at length escaped from the skin of the tardigrade altogether.

Surely it is unsatisfactory science to consider a phenomenon like this "heterogenesis," and to label it "homogenetic pangenesis in tardigrades!"

The "transmutations" of the living protoplasm of vegetables, given in evidence of the hypothesis, are all subject to the same defect. That is everywhere *assumed*, which at least *might* have presented another explanation, had all the possibilities of error been eliminated, and continuous observations been made. I select, to illustrate this, fig. 11, Pl. CXL., from Mr. Chantrell's drawings. This object I saw in the living state. It is described as a "curious fungoid growth, found in a trough with Anacharis, some weeks old." Now it appeared to me, in the living state, to be quite impossible, apart from unbroken observation, to determine whether the amœbæ or amœboid masses, *a a*, attached to the arms of the fungus, were anything more than preying upon it and devouring it as food. But the presumption was that the protoplasm of the fungus was being "transformed" into amœbæ. But can such an inference be maintained? We know how easily the minute spore of the smaller creatures, as, for example, the monads, may penetrate into the very substance of growing vegetable forms, and become inhabitants of the cells. Look, for instance, at the fact of ditomaceæ being absorbed from infusorial soil into the roots of plants, and being built up unaltered into their stem substance.* And that spore may germinate in the cells of plants is fully attested. Besides, what can be more uncertain than the organism to which an amœboid condition of protoplasm belongs? It

* "Silliman's American Journal," May 1876.

might, indeed, be merely a phase in the life development of the "curious fungoid growth" itself. All these possibilities, and a hundred others, in every instance should be most crucially considered before the semblance of a case for "heterogenesis" could even be presented in a strictly scientific form.

But Dr. Bastian has neglected such precautions, and has adopted the observations of others, which are even more invalid. How serious the resulting errors are, in only one direction, has been plainly shown by Professor H. L. Smith,* of America, whose competence to write critically on the subject of diatomaceæ will not be disputed. He has given an absolutely destructive detailed criticism of every important instance of the reputed transmutation of something else into diatoms which Dr. Bastian has presented, and brings out clearly the mistake of attempting to infer the "heterogenetic" origin of vital forms of whose ascertained history the observer was ignorant. Professor Smith says: "I have probably witnessed more of the phenomena of conjugation and growth than any other person, and can affirm, without fear of being disproved, that . . . . any kind of transformation of Pediastreæ or Desmids into Diatoms never has happened—nay more, never will happen." "I look," he continues, "more particulary to the evolution of diatoms, fully convinced, however, that the errors of misinterpreting what he (Dr. Bastian) saw are quite as great with the desmids as with the diatoms." For example, fig. 12, Pl. CXXXIX., is a reproduction of one of Dr. Bastian's figures, which he declares represents, at *e*, *e'*, the "heterogenetic" origin of diatoms from the *Cladaphora* filament A. Professor Smith says, poor as the cut is, we easily recognise the "pedunculated diatoms" as "*Acanthes exilis in its normal condition!*" In fact, it constantly grows naturally thus on *Cladophora*, *Vaucheria* and other algæ. But because Dr. Bastian *was not aware of this*, he took the observation as a fine illustration—which in view of the facts we have no objection to admit—of heterogenesis. Whereas, "if it had been allowed to live it would have continued the process of self-division until finally . . . . a new sporangium would have formed the commencement of a new series."

Again, Dr. Bastian affirms the small forms figured at *l l'* (fig. 12) are algoid vesicles budded off from Vaucheria, and that they "gradually become converted into different kinds of diatoms." And further, "These bodies increased in size, and it soon became obvious that they were young *Naviculæ* (*l l'*); the exact pattern assumed in the early stages is subject to much variation,

* *Vide* "Archebiosis and Heterogenesis," the "Lens," Jan. 1873; and "Quarterly Journ. Micro. Science," vol. xiii. p. 357; and note by Mr. Archer, *ibid.* p. 313.

and several different diatoms *seemed* to be produced corresponding to these initial forms, *m m'* " (fig. 12). "This," says Professor Smith, "would be wonderful if true; but not only is there no evidence that actual diatoms *did* come from the vesicles of *Vaucheria*, but any one familiar with the observation of living Diatoms can tell *where* they did come from. . . . . They were in the gathering . . . . . and made their appearance out of the *débris* . . . . as we know they will do under the influence of light. . . . . But, besides, *Diatoms do not grow by increase of size;* there are no such things as broods of young frustules. . . . . The late Dr. Greville . . . . fully agreed with me in this."

"Further, fig. 13, Pl. CXL., is a copy of another illustration given in the "Beginnings of Life." It is declared to represent the "resolution of Euglena into diatoms." It is said concerning it that "the whole of the contents of an euglena *seemed* to have been resolved into distinctly striated *Naviculæ* . . . . *although the earlier stages of the transformation were not seen* (*!*) I have *no doubt* that the diatoms originated in this way." Upon this Professor Smith observes: "He (Dr. Bastian) is more easily satisfied that a euglena can transform into a diatom, which possesses a wonderful silicious and beautifully-sculptured epiderm, than he is that bacteria come from air-germs;" and then he clearly shows that the group of Naviculæ seen in fig. 13 are simply a group that were devoured, and their protoplasm digested, by an amœba. They constantly are ejected in this way from the body of the amœba after the nutrition has been abstracted, and look like an encysted mass with an envelope complete; and even when treated with acids, although the envelope disappears, the frustules still adhere. And Professor Smith has "slides as well as material showing this in abundance."

All this, it may be presumed, is capable of suggesting two things: 1. The danger of attempting to discover *new* modes of "genesis" until we have made ourselves acquainted with the old ones; and 2. That "heterogenesis" is not even a scientific *hypothesis*, for the "facts" on which it is founded have not received scientific investigation.

But hitherto I have dealt with the question as a whole. I may now touch a point with which I have endeavoured to make myself specially familiar. Dr. Bastian claims that bacteria are constantly being "transformed" into *monads*. This, it is affirmed, takes place in what is known as the "proligerous pellicle," or scum which very rapidly forms on the surface of infusions. This pellicle is formed chiefly of bacteria; but it *may* contain every variety of form possible to a given infusion, in the earlier stages of their development. It is worthy of remark that there is not a single instance throughout in which Dr.

Bastian furnishes a correct illustration of a true pellicle. It is always drawn too discreet and too uniform. The bacteria are always matted together in the closest manner, and millions of the minute germs of diverse lowly life-forms might be interspersed in that portion of the pellicle which constitutes the "field" of even the highest powers; and I say advisedly that no microscopist in the world could ever distinguish them.

It is now established that the monads produce germs or spore —and they are extremely minute. If such germs should be interspersed in a given pellicle, their earliest development could not be seen at all; and when it had reached a certain stage it would only be visible under proper and definite conditions. But when the developing monad germ had reached say the size of a bacterium in the pellicle, I presume that Dr. Bastian would not claim any ability to distinguish the one from the other. Nevertheless fig. 14 is the copy of a drawing accepted by him from Pouchet as displaying the origin of "*Monas lens*." *a* is supposed to represent the pellicle, and the small aggregations in it are taken as the initial stage of the "transformation" of these organisms into monads; this is said to go on, until a stage like that seen in *b* is reached; and eventually a flagellated and complete form results, as seen at *c*. And this is presented as a case of heterogenesis. Now the fact is that the monad which is meant to be depicted here (but which is badly drawn) arises in a germ—quite invisible unless looked for under special conditions, and not likely to be seen, with the appliances used, *in a pellicle*. But when this and other monad germs develope, as is constantly the case, in the pellicle, its growth and expansion, and probably other causes, modify, more or less plainly, the immediately surrounding portions of the pellicle, giving it an appearance similar to, but too strongly depicted at, *a* (fig. 14), the point indeed in which the bacteria are supposed to be transforming into monads. After this the germ rapidly progresses, the flagellum is acquired, and the perfect monad swims away. In fact it is no more a case of "heterogenesis" than is the birth of a humming-bird or an ox. And this explanation will apply in every instance; the said "transformations" are slightly altered conditions of the pellicle, resulting from the natural growth of interspersed monad germs.

But nowhere has the egregious mistake of this observer's method so much impressed me as in the illustration on p. 220 of vol. ii. of his "Beginnings of Life." I reproduce as much of it as is required in fig. 15; *a a* are monads which are supposed to have originated in the bacteria. At *c c c* these monads are ("heterogenetically") changing, or changed into *amœbæ*. At *d d* these amœbæ are seen minus the flagella, in an active and a still condition. At *e*, *f*, *g* they have become encysted;

and at *k*, *l*, *m* they are seen to resolve themselves into bacteria again!

Now, if Dr. Bastian had in this case only watched more *continuously*, and with the care required, he would have found that he had here the *complete life-cycle of a monad*; and by doing so would have handed over a useful fact to science. But as it is, the truth is absolutely obscured.

The facts may be briefly glanced at. A monad life-history, in all essential respects agreeing with the "heterogenetic" stages just given, was worked out and published in the "Monthly Microscopical Journal."* It originates in a definitely discovered and carefully followed germ. In fig. 16 is a drawing of a germ arising in the pellicle. This was under a magnification of 3,500 diams.; and in the drawing all is reduced to one-fourth, except the germ just commencing to develop, which is exaggerated, even at the full magnification, for the sake of clearness, and shown at *a*. Fig. 16_2, a drawing made on the same conditions, shows the early development of the germ. Fig. 17 is a fully developed monad. After a greater or less length of time spent in multiplication by fission, it becomes *amœboid*, as seen in fig. 18. This may be compared with *c c c*, fig. 15. This amœboid state becomes very general, and two meet, as at fig. 19, and instantly unite, the blending going on until the two forms are united into one, as seen in fig. 20, each of which may be compared with *d* (lower figure), fig. 15. Encystment now rapidly ensues, and is in progress at fig. 21, which may be put beside *d* (upper figure), fig. 15; and this is complete in fig. 22, perfectly comparable to *e*, *f*, *g*, fig. 15. This cyst eventually pours out—certainly not bacteria—but germs, which were watched continuously into the parent form. Thus what Dr. Bastian supposed was the "transformation" by "heterogenesis" of one vital form into another, was in fact only a series of stages in the metamorphosis through which a monad with an ascertainable history was passing. While in the same journal† another such history is given in which the organism, after passing through similar preceding changes, becomes a cyst, which pours out *living young*.

Thus, wherever certain knowledge is brought to bear upon the reputed cases of "heterogenesis," they are easily shown to be erroneous and misleading, and afford no foundation for the superstructure that has been raised upon them. As no sound philosophy can be opposed to the conception of a continuity in Nature, and no true science can object to its discovery, but must

* "Researches on the Life-History of a Cercomonad." By Dallinger and Drysdale. Vol. x. p. 53.

† Vol. xi. p. 7.

rejoice in it, so no true biologist would reject "heterogenesis" for its own sake, or because it opposed what seemed to be established facts; but as it would be retarding the development of the most sacred of all things—Truth—to accept as evidence of Nature's continuity between the living and the dead Dr. Bastian's proffered evidences—even the latest—of "abiogenesis," so it would be weakness without a name to accept "heterogenesis" on the erroneous facts and equally erroneous inferences of its principal if not its only advocate.

EXPLANATION OF PLATES CXXXIX. AND CXL.

FIG. 1. A group of minute rotifers, which, their history being unknown, might have been supposed to have been transformed *volvoxes*.

FIG. 2. The above rotifer magnified 80 diams., and identified with *Notommata parasita* (Ehr.).

FIG. 3. The same rotifer as a parasite within the volvox, the latter being slightly out of focus to sharpen the image of the rotifer.

FIG. 4. A new and unknown monad, found in a pond at Sandhurst.

FIG. 5. *Dinocharis Collinsii*, which suddenly appeared with the disappearance of the monad (fig. 4).

FIG. 6. A metamorphosis of fig. 4.

FIG. 7. Profile of fig. 5, showing a grotesque similarity between figs. 6 and 7.

FIG. 8. The body of a tardigrade, said by Dr. Bastian to have resolved its "total internal substance" into germs which are said to be capable of development into tardigrades.

FIG. 9. A similar phenomenon seen by Mr. G. F. Chantrell, in which the germs were followed out and became stentors. *9a*, *9b*.

FIG. 10. The hollow skin of a tardigrade, showing that it may become the accidental resting and hatching place of small eggs or spore; *b* an egg, and *c* a rotifer (*Monura dulcis*), emerged from it.

FIG. 11. Amœba at *a a*, supposed to have arisen from the fungus which appeared upon anacharis (G. F. Chantrell).

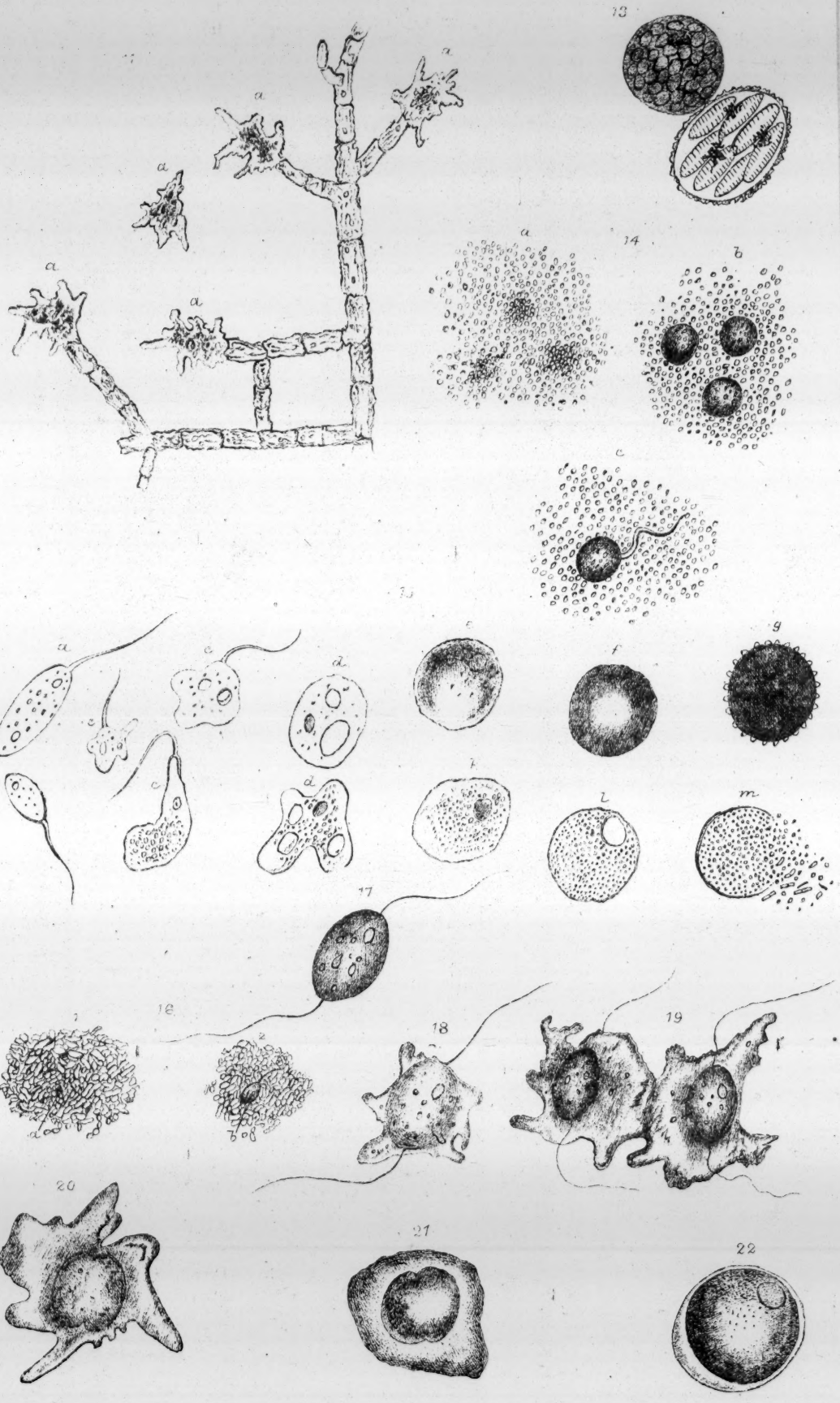
FIG. 12. Supposed case of "heterogenesis" of diatoms from *Cladophora* and algoid corpuscles, *e e'*, *l l'*, *m m'*, but shown by Professor Smith to have arisen naturally in this position, which is their habit.

FIG. 13. A supposed case of "heterogenetic" production of Naviculæ from Euglena, but shown by Professor Smith to be a cluster of diatoms that had been devoured and ejected by an amœba.

FIG. 14. Supposed origin of *Monas lens* in transformed bacteria.

FIG. 15. The reputed "heterogenetic" change of monads into amœba and back again to bacteria (Bastian).

FIGS. 16 to 22. The changes in fig. 15, supposed to be heterogenetic, explained by the ascertained metamorphoses of the monad here depicted.



Illustrations of mistaken "Heterogenesis"

ASTRONOMY IN AMERICA.

By RICHARD A. PROCTOR.

DURING my visits to America in 1873–74 and 1875–76, I was led from time to time to notice with interest the progress and promise of astronomical science in America. My own special purpose in visiting America on these occasions partly brought these matters to my attention. The circumstance that in a country so much more thinly peopled than Great Britain, it should be possible not only to obtain audiences for lectures on such a subject as astronomy, but to obtain more and better and larger audiences by far than could be obtained during a lecture season in England, for any single scientific subject whatever, appeared to me in itself sufficiently remarkable. At a first view this might have been referred simply to the fact that the Americans are a lecture-loving people, preferring the quick and ready method of learning the more striking facts of a subject from a verbal exposition, to close study and application. But I soon perceived that something more than the mere desire for superficial knowledge was in question. The number of persons making close inquiry into the subject was nearly always greater (even in proportion to the much greater audiences), than in England. That still more select section of every audience, the actual workers and observers, I also found to be correspondingly large; while again and again I met with what in England is certainly very unfrequent—cases, namely, in which persons not engaged professionally in the study or teaching of astronomy had privately worked so zealously and so ingeniously in astronomical research as to have effected original discoveries of considerable interest.

I do not propose, however, to enter here into an account of these experiences of my own. To do so would indeed be a welcome task to me, as enabling me in some degree to express not only my sense of the interest taken by Americans in science, but also my recognition of the unvarying kindness with which I was personally received. At Boston, New York, Philadelphia,

Washington, Brooklyn, St. Louis, Cincinnati, Baltimore, Chicago, Columbus, Louisville, Kansas, and Minneapolis, and, in fact, at all the cities and towns which I visited, I received a generous and kindly welcome from the community, accompanied by acts of personal kindness from individuals, which I shall always hold in grateful remembrance. But this would not be the place to attempt the task—in any case no easy one—of attempting to express my sense of American kindness and hospitality. My present purpose is to indicate simply the remarkable progress made by Americans in astronomical science during the last half-century.

Fifty years ago there were few telescopes and no observatories in America. It was not greatly to be wondered at that the nation should not up to that time have given any great degree of attention to scientific matters. The proportion of the population having leisure for scientific and especially for astronomical research was but small, and the government had matters of more vital importance to attend to than the erection of observatories. For several years the attention of Congress had been called to the necessity of a national observatory; but when President Adams, in 1825, made a special appeal to this effect, his proposal met with ridicule and disfavour.

The first action towards the initiation of astronomical research in America bears date March 1810, when it was proposed in Congress (by Mr. William Lambert, of Virginia), that a first meridian should be established for the United States (the meridian of the Capitol at Washington being selected), in order to obviate the "confusion already existing in consequence of the assumption of different places within the United States as first meridians, on the published maps and charts" in the country. The proposition was not at once acted upon. In July 1812 we find Mr. Monroe, then Home Secretary of State, indicating its astronomical bearing. "In admitting," said he, "the propriety of establishing a first meridian within the United States, it follows that it ought to be done with the greatest mathematical precision. It is known that the best mode yet discovered for establishing the meridian of a place is by observations of the heavenly bodies; and that, to produce the greatest accuracy in the result, such observations should be often repeated, at suitable opportunities, through a series of years, by means of the best instruments. For this purpose an observatory would be of essential utility. It is only in such an institution, to be founded by the public, that all the necessary implements are likely to be collected together; that systematic observations can be made for any great length of time, and that the public can be made secure of the results of the labours of scientific men. In favour of such an institution it is sufficient to remark that every nation

which has established a first meridian has also established an observatory." Mr. Lambert brought in a bill proposing the erection of such an observatory in 1813; but nothing more was done until 1815, when the memorial on which the bill of 1813 had been based was referred to a select committee. No steps were then taken, however, to carry a bill. In November, 1818, a third memorial from Mr. Lambert was presented, and referred to a select committee; but the resolution asked for was not finally passed until March 3, 1821, when Mr. Lambert was appointed by the President, "to make astronomical observations by lunar occultations of fixed stars, solar eclipses, or any approved method adapted to ascertain the longitude of the Capitol from Greenwich." In December 1823 Mr. Lambert, in a report of his labours, gave for the longitude of the Capitol 76° 55′ 30″ ·54, closing his report with a strong appeal for the erection of an observatory.

Two years later, President Adams urged on Congress the establishment of a national observatory as part of a wider scheme for the advancement of knowledge. His remarks on the astronomical portion of his scheme serve well to show the position of astronomy in America half a century ago. "Connected with the establishment of a university," he says, "or separate from it, might be undertaken the erection of an astronomical observatory, with provision for the support of an astronomer to be in constant attendance on the phenomena of the heavens, and for the periodical publication of his observations. It is with no feeling of pride as an American that the remark may be made, that, on the comparatively small territorial surface of Europe there are existing more than one hundred and thirty of these lighthouses of the skies; while throughout the whole American hemisphere there is not one. If we reflect for a moment upon the discoveries which in the last four centuries have been made in the physical constitution of the universe by means of these buildings, and of observers stationed in them, shall we doubt of their usefulness to every nation? And while scarcely a year passes over our heads without bringing some new astronomical discovery to light, which we must fain receive at secondhand from Europe, are we not cutting ourselves off from the means of returning light for light, while we have neither observatory nor observer upon our half of the globe" (!) "and the earth revolves in perpetual darkness to our unsearching eyes?"

In March 1826 a bill "to establish an observatory in the district of Columbia" was brought before Congress and read the first and second time, but the House Journals show no further trace of it. This bill was due to the recommendations of Mr. Adams, who did not relax in his efforts to secure the erection of a national observatory, though delays and disappointments

occurred which might well have exhausted his energy, seeing that the dates of his renewed and for awhile useless appeals were 1836, 1838, 1840, and 1842.

Passing over many circumstances in the history of these transactions, not as being without interest, but because space will not permit of their being presented here, we may proceed to the time when the actual erection of the buildings was commenced. This was in 1843, or no less than thirty-three years after the plan for an observatory was first proposed, so that fully one half of the period which has elapsed since Lambert of Virginia first called his countrymen's attention to the necessity of establishing a national observatory was lost in discussions and delays. At the close of September 1844 the new building was ready for occupancy, and the instruments were adjusted.

From 1844 to 1861 the Washington Observatory was under the superintendence of Lieutenant Maury. In September 1846 the first volume of Observations was issued. Its value has been thus described by an impartial and competent judge. "Besides a fair amount of observations with the two transit instruments in the meridian and the prime vertical, and those with the mural circle, it contained various important investigations of the errors and corrections peculiar to the instruments: Professor Coffin's masterly discussion of the adjustments of the mural circle, and his expansion of Bessel's Refraction Tables; Walker's investigation of the latitude of the observatory, and his comparison of the standard thermometers; all of great value."

In the second volume reference was made to the discovery of Neptune, and the success of Mr. Walker, one of the assistants, in detecting amongst Lalande's observations two of Neptune on May 8 and 10, 1795, when the planet was observed and recorded as a fixed star. "Astronomers were thus furnished with an observation of Neptune made 52 years before, which afforded the means of a most accurate determination of the orbit, and enabled the superintendent of the American Nautical Almanac to publish an ephemeris of the new planet two years in advance of all other parts of the Almanac. *The observatory was first brought into prominence by these researches.*" In October 1849 Lieutenant (now Rear-Admiral) Davis wrote as follows to the Hon. Secretary of the Navy on this subject. "The theory of Neptune belongs, by right of precedence, to American science. In connection with its neighbour, Uranus, it constitutes an open field of astronomical research, into which the astronomers and mathematicians of the United States have been the first to enter, and to occupy distinguished places." Deprecating heartily, though I do, all reference to priority or nationality in such matters, as opposed to the true scientific spirit, I cannot but note how Professor Newcomb, by his admir-

able researches into the theory of Uranus and Neptune, has fulfilled the hopes thus expressed nearly a quarter of a century before his labours were brought to a successful termination.

The work of the observatory thus happily inaugurated was prosecuted steadily till 1861, when Commander Maury left Washington to join the cause of the Confederate States. During the greater part of the war the observatory was under the charge of Captain Gilliss, who died on February 9, 1865. "It has been noted as a strange coincidence of circumstances," says Professor Nourse, in the memoir of the observatory from which we have been quoting, "that the last morning of his life witnessed an announcement of results deduced at this observatory which had fulfilled his long deferred hope of determining the solar parallax by simultaneous observations in Chili and in the United States. This announcement would have been peculiarly gratifying to him because these results were from the joint activity of the two observatories, founded through his exertions, five thousand miles apart."

From 1865 to 1867 the observatory was under the superintendency of Rear-Admiral C. H. Davis, and from 1867 to the present time it has been under that of Rear-Admiral B. F. Sands. Without further considering the work accomplished at the observatory itself, which has partaken of the general character of official astronomical research, we may consider here some of the special astronomical occasions at which the observers trained at Washington have assisted.

The total eclipse of August 7, 1869, was closely observed by parties from the observatory. Professor Asaph Hall and Mr. J. A. Rogers, proceeded to Alaska; Professors Newcomb, Harkness, and Eastman, to Iowa; and Mr. F. W. Bardwell, to Tennessee. The observations made on this occasion were of great value and interest. The solar prominences had had their real nature determined during the eclipse of August 1868; and the American observers were not content to repeat the observations then made, but extended the method of spectroscopic analysis to the corona. They also obtained photographs of the coloured prominences. The work accomplished by the Washington observers, together with the observations made by Dr. Curtis, Mr. J. Homer Lane, of Washington City (Ia.) and Mr. W. S. Gilman, jun., of New York, and Gen. Myer, U.S.A., form a quarto volume of 217 pages, with twelve illustrations. Of this valuable and interesting volume three thousand five hundred copies were printed by joint resolution of Congress.

The superintendent of the Washington Observatory was not content with this. "Believing that the experience of its officers in their observations of the eclipse of 1869 should be availed of for the further elucidation of the subjects involved in such

phenomena, he addressed the Navy Department upon the subject of their employment in Europe in observing the eclipse of December 1870; the Department promptly detailed the professors who had been the observers of the previous year;" and it was doubtless through the energy thus displayed by Rear-Admiral Sands, that other skilful American astronomers were enabled to cross the Atlantic for the purpose of observing that important eclipse. Unfavourable weather prevented observations of this eclipse at some of the best stations, but the American observers succeeded in establishing the accuracy of the observations made in 1869; and to them must be attributed in large part the definite demonstration of the fact, which though now admitted was then much disputed, that the corona is a solar phenomenon, and not due to the illumination of our own atmosphere only.

The part taken by the Washington Observatory in preparing for and co-operating in the observation of the transit of Venus, on Dec. 8, 1874, is too recent to need full description in this place. I may be permitted, however, to dwell with special commendation on the manner in which American astronomers devoted themselves at that time to a task which they might fairly have thought the business of their European brethren. A transit of Venus is to occur in 1882 which will be specially American, being visible wholly or in part from every portion of the United States; and if America had reserved her energies for that occasion, no complaint could reasonably have been made. It was indeed the prevalent idea in Europe that that would be the course she would adopt. But with singular generosity and scientific zeal, she not only devoted to the work of observing the earlier transit a sum largely exceeding the amount granted by any other government (and nearly twice as large as Great Britain paid), but undertook some of the most difficult portions of the work, which otherwise would have been left unprovided for. I cannot but recall with a feeling of something like personal satisfaction (though conscious that such a feeling ought to find no place in the mind of the true student of science) the gratification with which I welcomed the announcement, early in 1873, that America had undertaken to occupy positions, the importance of which I had long pointed out, but which, but a fortnight before that announcement reached Europe, had been confidently described as astronomically inferior and geographically unsuitable. The pleasure I then felt was only surpassed by that which I experienced subsequently, when news received from the various observing stations showed that at those just mentioned were achieved some of the most important successes of the occasion.

Another noble contribution made to science at Washington has been the erection of the splendid refractor, 26 inches in

aperture, which is now the chief equatorial of the observatory. America is fortunate in possessing in Alvan Clark the greatest living master of the art of constructing large object-glasses of good definition. He had already constructed a telescope 18 inches in aperture for the observatory at Chicago; but by the contract negotiated with him in August 1870 by Professor Newcomb, he was called on to achieve a far more difficult task in the construction of a telescope of 26 inches clear aperture. He has successfully accomplished this task, and the telescope has already obtained good results under Newcomb's skilful management. The most important of these is an extensive series of observations of the satellites of Uranus and Neptune, made with a view of determining the elements of their orbits and the masses of the planets round which they circle. The observation of the two Uranian satellites, Ariel and Umbriel, discovered by Lassell, and of the Neptunian satellite also discovered by him, must be regarded, on account of the extreme difficulty of observing these bodies, as a very valuable contribution to astronomy. It is pleasant to notice that Newcomb has been able most thoroughly to confirm the accuracy of Lassell's work in Malta, the mean motions of Ariel and Umbriel deduced from the Malta observations being so accurate that, says Newcomb, "they will probably suffice for the identification of those objects during several centuries." Although no systematic search has been made for new satellites of Uranus, yet enough has been done to show, "with considerable certainty," that at least the outer satellites supposed to have been seen by Sir W. Herschel "can have had no real existence" (as satellites, that is to say).

Before passing to the brief consideration of the work accomplished in some of the other American observatories, we must fully admit the justice of the remarks made by Professor Nourse in closing his memoir relating to it. "The position now accorded to it," he says, "by the free tributes of scientific men in the Old World as well as at home, is not without honour to our country; and this notwithstanding the comparatively recent founding of the institution, and the as yet limited appropriations sustaining it. It may, therefore, justly claim a yet more generous support; and the pledge may be safely made that if thus supported and efficiently directed, it will make returns yet more gratifying to national pride, and (which is a matter infinitely more important) advancing the highest aims of scientific research. What shall be its future records of success must remain with the support extended by the government and the fidelity of those who are entrusted with its administration."

The actual commencement of astronomical observation in America belongs to a much earlier period than that at which the Washington Observatory was erected. The first telescope

used for astronomical purposes in America was set up at Yale College forty-six years ago. The first observatory, however, properly so-called, was erected at Williams College, Mass., in 1836. The next was the Hudson Observatory, established in connection with the Western Reserve College, Ohio, under the charge of Professor Loomis (now of Yale), whose works on astronomy are deservedly held in high esteem in this country as well as in America. Next in order of time came the Observatory of the High School at Philadelphia, which achieved distinction under the able management of Messrs. Walker and Kendall. The West Point Observatory was next established, and placed under the care of Professor Bartlett. All these preceded the Washington Observatory.

Soon after the Washington Observatory had been erected, an observatory was built at Cincinnati. Its history illustrates well the way of carrying out such work in America, when the government does not take the work in hand. The idea of erecting an important observatory in Cincinnati was first entertained by Professor Mitchel, then professor of mathematics at Cincinnati College. He proposed to attempt the task without any aid from the general or state government, by the voluntary contribution of all classes of citizens. To ascertain whether any interest could be excited in the public mind in favour of astronomy, he delivered in the spring of 1842 a series of lectures in the hall of Cincinnati College. With truly American ingenuity he devised a mechanical contrivance, by help of which telescopic views in the heavens were presented with a brilliancy comparable with that "displayed by powerful telescopes." These lectures were attended by large audiences, and, I may add in passing, that the interest which they excited is to this day well remembered in Cincinnati—no small proof of Professor Mitchel's power as a lecturer.* The last lecture of the course was delivered in one of the great churches of the city (a thorough American and sensible proceeding), and at the close Professor Mitchel submitted to the audience, consisting of more than two thousand persons, his plan for erecting a first-class observatory, and furnishing it with instruments of the highest order. He promised to devote five years of faithful effort to accomplish this task. The following course was then suggested:—"The entire amount required to erect the buildings and purchase the instruments should be divided into shares of twenty-five dollars; every shareholder to be entitled to the privileges of the ob-

* The same remark applies to the lectures which he subsequently delivered in New York, New Orleans, Boston, Brooklyn, and other large cities. It is almost impossible to over-estimate the service thus rendered by Professor Mitchel to astronomy in the United States.

servatory under the management of a board of control, to be elected by the shareholders. Before any subscription should become binding, the names of 300 subscribers should be first obtained. These 300 should meet, organise and elect a board, who should thenceforward manage the affairs of the association." In three weeks the 300 subscribers had been obtained, without calling any public meeting, and merely by quiet visits in which the nature of the scheme was described and explained. Then officers were elected, a directory formed, and Mitchel was sent "to visit Europe, procure instruments, examine observatories, and obtain the requisite knowledge to erect and conduct the institution, which it was now hoped would be one day reared."

When Mitchel returned four months later, a great change had occurred in the commercial affairs of America. "Everything was depressed to the lowest point," and it was with great difficulty that a sum of 3,000$ was collected and remitted to meet the first payment for the telescope of 12 inches aperture ordered of Merz. The best place for the observatory was a hill-top rising 400 feet above the level of the city. On offering to purchase this from Mr. Longworth, to whom it belonged, Professor Mitchel was directed to select and enclose four acres, which Mr. Longworth presented to the association. On Nov. 9, 1843, the corner-stone of the pier which was to sustain the great refracting telescope was laid by John Quincy Adams, who undertook the long (and then difficult) journey from Washington to give this proof of his interest in the cause of astronomy. When, in May 1844, the great telescope was paid for, the funds of the association were exhausted, and the estimated cost of the building amounted to more than 7,000$. In this difficulty a simple but again perfectly American plan was followed. Mechanics and others were invited to subscribe for stock in the Astronomical Society, paying their subscriptions with work. In six weeks not less than one hundred hands were at work on the hill-top and in the city. The stone of which the building was erected was quarried from the grounds of the society. The lime was burned on the hill, and every means was adopted to reduce unnecessary expenditure. Payment for stock was received in every possible article of trade; due bills were taken, and these were converted into others which would serve in the payment of bills. In this way the building was reared, and finally covered in, without incurring any debt. But the conditions of the bond by which the lot of ground was held required the completion of the observatory in June 1845. It was seen to be impossible to carry forward the building fast enough to secure its completion by the required time without incurring some debt. "My own private resources," proceeds Mitchel, "were used in the hope that a short time after the

finishing of the observatory would be sufficient to furnish the funds to meet all engagements. The work was pushed rapidly forward. In February 1845 the great telescope safely reached the city; and in March the building was ready for its reception." Unfortunately, just at this time, when his private means were exhausted, Professor Mitchel's professorship was brought, in a very summary manner, to a temporary close, in consequence of the college edifice being burned to the ground. To recruit his means without abandoning the cause of astronomy, he gave courses of lectures in the chief cities of the United States, meeting with well-deserved success.

The observatory thus erected achieved useful, though not very striking results. An observatory which was erected a year or two later took so quickly the leading position, so far as the actual study of the heavenly bodies was concerned, that the progress of the Cincinnati astronomers, as indeed of most of the astronomers of the United States, received less attention than otherwise might have been the case. I refer to the Observatory at Harvard (Cambridge, Mass.). Here one of the first equatorials ever made by Merz was erected; and by means of it W. C. Bond and his son Geo. P. Bond made highly interesting additions to astronomical knowledge. The seventh satellite of Saturn (eighth and last in order of discovery) was detected, the dark ring rediscovered and found to be transparent; important drawings of nebulæ were made, and many other observations were effected, under the administration of the Bonds. Later, under Professor Winlock, the Harvard Observatory has been distinguished by the excellence of the mechanical arrangements adopted there, and by M. Trouvelot's admirable drawings of solar spots and prominences of the planets Jupiter and Saturn, and of various details of lunar scenery.

In passing, I may note that at Harvard, as indeed elsewhere in America, others than professed astronomers have achieved very useful astronomical work. As Professor Mayer, of the Stevens Institute, Hoboken, has turned his marvellous ingenuity in devising new methods of physical research to astronomical inquiries, so Professor Cooke of Harvard, whose special subject is chemistry, made a most important astronomical discovery, which has since been ascribed to Janssen, who, later (though independently and by another method) effected it. Professor Cooke made a series of observations on those bands in the solar spectrum which are due to our own atmosphere, with the object of ascertaining whether they are due to the constant constituents of the air, or to the aqueous vapour which is present in the air in variable quantity. Combining hygrometric with spectroscopic observations, he found that when the air is moist these bands are more clearly seen than when the air is dry, and

by systematic observations so definitely ascertained this relation as to prove beyond all manner of doubt that the bands are due to aqueous vapour. Unfortunately, though his results were published in America, they were not published in such a way as to attract notice in Europe, and accordingly European astronomers remained ignorant of the most important fact discovered by Cooke until they had rediscovered it for themselves.

The Observatory at Ann Arbor, Michigan, was erected in 1854, chiefly through the exertions of Chancellor Tappan, of the Michigan University. Dr. Brünnow, our present Astronomer Royal for Ireland, was for a long time director of this observatory. It is at present under the able control of Professor Watson, who has added nearly a score of planetoids to the known members of the solar family.

The Observatory of Dartmouth College, Hanover, N.H., illustrates in a remarkable way the energy and zeal with which college observatories are managed in America. It would be difficult to name any observatory in this country where observations of greater interest, as respects the physics of astronomy, have been made than those effected by Professor Young with the 9-inch telescope constructed by Alvan Clark for the Dartmouth College; or than the supplementary observations made by Young with a powerful telescope conveyed to an elevated pass in the Rocky Mountains. Amongst his results may be specially mentioned—first, the observations of the most remarkable solar outburst yet witnessed, an outburst during which the glowing hydrogen of the prominences was driven to a height of at least 200,000 miles from the surface of the sun; and, secondly, the identification of more than 250 lines in the spectrum of the solar sierra.

And as the most interesting and characteristic observations yet made upon solar prominences are due to Professor Young of Dartmouth Observatory, so the most accurate and detailed drawings yet made of sun-spots are those by Professor S. Langley, of the Alleghany Observatory, near Pittsburgh.

At Chicago, a very fine telescope, 18 inches in aperture, by Alvan Clark, has been erected; but, owing to pecuniary difficulties consequent on the great fire (followed by the commercial depression which has recently affected the United States), that observatory has suffered considerably from the want of a properly remunerated director. The Astronomical Society of Chicago has done its best to set matters straight, but differences have arisen which have marred their efforts. In the meantime Mr. S. W. Burnham, of Chicago, has shown admirable zeal and skill in the systematic observation of double stars, having discovered and measured more than 450 of these objects (all of a delicate and difficult nature).

But, indeed, it would be hopeless to attempt, in the short space available to me here, to give any sufficient account of the labours of American astronomers, whether attached to government or state observatories, or working independently. Of the latter, and in my opinion not the least important class, I need cite only Drs. Rutherford and H. Draper, the former of whom. besides making other extremely important contributions to astronomy and physics, has produced celestial photographs admittedly better than any obtained on this side of the Atlantic, while the latter at an earlier period achieved results in celestial photography which were far superior to any obtained at that time, or for many subsequent years. The advice and assistance rendered by Dr. H. Draper to the astronomers to whom was entrusted the preparations for the recent transit, was most deservedly commemorated in a medal which the American government honoured itself by awarding to him.

The most striking feature in the contributions made by Americans to astronomy appears to me to be the skill shown in noting the essential points to be aimed at, and the fertility and readiness of resource exhibited as the work proceeds. In England, students of astronomy are too much in the habit of following conventional rules and wasting time over unnecessary preliminaries. An American astronomer notes that some particular observation is wanted, and directs his efforts to making that observation, not considering it necessary in the first place to go over ground already repeatedly traversed by others.

I have been sometimes asked whether officialism is as rampant in America as in England in matters scientific. American scientific officials have assured me that it is, or rather (for they have not worded the matter precisely in that way) they hold that official science is properly (as they consider) paramount in their country. I was gravely assured in Washington, for instance, that the course which I had pursued in England with reference to the suggested official schemes for observing the transit of Venus in 1874 would never have been tolerated in America, despite the fact that the course actually followed by American official science was precisely that which I had advised. It was the *principle*, so an eminent American official scientist assured me, which was in question, and no American would have been suffered to oppose as I did the course advised by the chief official astronomer. What would have happened to such an unfortunate was not clearly indicated; and I must confess that all I heard outside official scientific circles in America suggested to me that any mistake made by official science would be commented upon even more freely in America than in England, and quite as safely. In fact, I had reason to believe that the warmth of my own welcome in America was in no small degree

due to the fact that having first proved the justice of my views, I had not been afraid to maintain them publicly against the powers that were until the proper course was adopted.

One other point remains to be noticed—the influence, namely, of religious scruples upon scientific progress and research in America. Here I must admit that I was somewhat disappointed. I expected to find America a long way in advance of England. But with some noteworthy exceptions, especially in the west, America seems to me to be behind England in this respect. It is only here and there in England—in the Bœotian corners, so to speak, of this country—that the community opposes itself to advanced scientific ideas to the same extent as in some of the leading cities of the United States. This is partly due to two opposite influences—the Puritan element of the American population on the one hand, and the Roman Catholic element on the other. Progress, however, is being steadily made in this as in other matters. Indeed, it has been rather because America began later to bestir itself in the encouragement of free search after truth, that she is at present behind England in this respect. Judging from experience in other matters, she will move rapidly now her progress has begun, and will soon occupy the position to be expected from the natural freedom and independence of the American mind. It need hardly be said, that in America as in Europe, such contest as arises from time to time between religion and science has its origin entirely from the side of religion. There, as here, religion (so-called) attacks and denounces discoveries inconsistent with the views which the orthodox had been accustomed to advocate; and there, as here, when there is no longer any choice, the orthodox quietly accept these discoveries as established facts, expressing a *naïve* astonishment that they should ever have been thought in the least degree inconsistent with received opinions.

ON THE PROGRESS OF AERONAUTICS.

By FRED. W. BREAREY,

Hon. Sec. to the Aeronautical Society of Great Britain.

TO the casual observer of a balloon which floats away from his presence into the dim distance, amidst the cheers of the crowd, and from thence into the solitude of an infinite space, it is hard to believe that its utility is doomed to the limit of mere flotation. He thinks that there is either an immense amount of apathy or else a lamentable display of ignorance among mechanical minds which prevents their energies being concentrated upon the navigation of the balloon. This is of course the popular judgment, yet it is only partially erroneous. The late Franco-German war afforded an opportunity for energy and engineering capabilities, and we know something of what balloons are capable, though perhaps not the uttermost, especially when accompanied by unlimited expenditure. The termination of the war interfered with certain designs, for the accomplishment of which M. de Fonvielle had escaped from Paris in a balloon. As he was the chief of the Aeronautical Department, he hoped to collect at Lille all the balloons which had left the French metropolis, and he himself came to England with the object of consulting as to the best means of aiding the return journey.

When the armistice was concluded six balloons had been collected at Lille, waiting for a favourable wind. By the aid of a small propelling force M. de Fonvielle believed that, starting with a fair wind, he would be able to deviate a few degrees from the current if necessary. It is highly probable that the return would have been effected, as Paris was the centre of a circle of investment of twenty miles diameter. With a favourable wind a sailor named Gally, with three others, left Paris on Nov. 12, with the intention of reaching Bordeaux, and they descended at Gondreville, *near* Bordeaux.

There is an anomalous incidence connected with the subject which would seem to favour the advocates of balloon propul-

sion, viz., that although water is about 800 times denser than air, yet the rarer atmosphere is capable of supporting the heavy bird, whilst the fish is about the same density as the element which it inhabits. They forget, however, that the currents to which the denser elements are liable, and with which the fish can cope with ease, are limited to eight, or at most to ten miles an hour, but where might the balloon be in a similar period? Possibly eighty miles away. Let us suppose a boat under the same conditions as often appertains to a balloon, viz. in a current of twenty miles an hour. The river, let us say, is five miles across, and we can propel our boat at the rate of five miles an hour, as M. Dupuy de Lome has lately accomplished in France with a balloon. The boat is accordingly propelled to the other side in one hour, but in that time it has drifted twenty miles down stream.

Now by powerful machinery it is just possible to propel a rigid construction like a boat against such a current with perfect impunity, but to attempt the same thing with a balloon is an abandonment of all scientific principles. Any attempt to force the balloon unnaturally into the condition of the boat must be made at the risk of a burst-up.

In the case of the boat, the thrust of the propeller is in the same line with the centre of gravity and displacement. In the case of a balloon the propeller works from a pendant car considerably below the centre of displacement.

If it were possible, therefore, to propel the balloon at a rate which bore a more suitable proportion to the force of the currents to which it is liable—let us say at the least twenty-five miles an hour—we may, without travelling with it to note the effect, just imagine the balloon anchored to the ground from the car, and a wind of twenty-five miles an hour blowing against it, which is precisely the same thing. The balloon would be forced into an inclination of about 60° out of the perpendicular.

The dimensions of a balloon intended to carry a propeller with the men or machinery to work it, could scarcely be less than 60 feet in height by 50 in breadth, and taking the mean it would be equal to a sphere whose largest section would contain about 2,372 square feet. If we take two-thirds of this, viz. 1,581 square feet—because remember that it is a compressible sphere—we shall obtain a surface upon which the whole force of the wind at twenty-five miles an hour will be expended; and at that rate, by Rouse and Smeaton's tables, the pressure upon each square foot exposed would be 3·075 lbs., equal to a total force of 4,861 lbs., or upwards of two tons. We find, therefore, the pressure to which a balloon of that capacity would be exposed were a motive power furnished capable of propelling such a balloon at twenty-five miles an hour; and we learn also

the angle of inclination which the car would assume relative to the balloon, the car in this case being in advance. Of course a more suitable shape of the balloon would modify these conditions, but only to produce other elements of difficulty which cannot be overcome. For instance, as we depart from the spherical shape of the gas-holder the buoyancy decreases and its weight increases! And particularly if some elongated form be constructed with conical or cigar-shaped ends, then struts must be employed to preserve its shape, and the extra weight to be thus sustained requires a balloon of such gigantic dimensions that, merely to inflate it, involves a cost quite out of proportion to any attainable result. No conditions except the exigencies of warfare would warrant the necessary expenditure. If any such conditions should ever attach to us as a nation, doubtless English talent will be equal to the emergency. Also if it be found desirable to escape from any besieged place, there will always be found sufficient of English body-linen, both male and female, for the whole population to float away, though perhaps in a makeshift manner.

The apparent anomaly before alluded to, of the lighter element and the heavy bird—the denser element and, by comparison, the light fish—is capable, however, of explanation, and its consideration will assist us in determining the conclusions at which the Aeronautical Society has arrived, viz. that flight is purely a mechanical action capable of imitation; that it is unassisted by air-cells or other contrivances for effecting levity, and that the balloon is incapable of being rendered useful to man as a means of locomotion, except in the way of waftage, and that this mode of progression in relation to the earth is capable of being materially assisted by some method of raising or lowering the balloon at pleasure, without loss of gas or ballast.

Let us suppose that the fish bears the same proportion of weight to its elemental medium as does the bird to the air. Judging from the fact that birds have been observed when shot to sink half their bulk in water, they may be taken to be about half the weight of water, and therefore they are about 400 times heavier than the bulk of air which they displace. If, then, the fish were equal in weight to the heaviest material of which we know, viz. platinum, it would still be light in comparison to the bird in the air. For instance, a cubic foot of platinum weighs 20,000 ozs; one of water weighs but 1,000 ozs.

We have estimated the bird as being half the weight of water, equal to 500 ozs. the cubic foot, whilst one of air is only about 1·285 ozs. Therefore the instant fall of the fish to the bottom could only be prevented by such appendages as wings, and the facility to manipulate them, which, however, the density of the element in which they exist entirely precludes.

The fish, then, would fall to the bottom like a lump of platinum without the ability to rise. It is the bird without wings, for the same thing would happen to the bird, did it not possess the ability to convert the force of gravitation into horizontal force by the manipulation of its wing surface, for it thereby covers and controls the weight of air, which in a given time is included, and passed over, within the breadth of the wing-tips. If necessary, gravitation may be still further diverted by the impact of the wing upon the air, because the resistance of the air bears a certain accelerated proportion to the rapidity with which it is displaced. This fact, so important to the hopeful student of aeronautics, is simply illustrated by the alternately slow and rapid waving of any light plane surface, such as a fan.

We have seen, then, that the fact of the specific gravity of the fish, and that of the element in which it disports itself, being nearly the same, presents no encouragement to the employment of the balloon as a means of locomotion, but it does afford encouragement for the adoption of the only suggestion made in its favour by the Aeronautical Society, because the fish possesses the power, within a narrow limit, of making itself lighter or heavier, and this is just the quality which we desire for the balloon, but to which, independently of throwing out ballast, or parting with gas which cannot be replenished, we have not yet attained.

In this direction, therefore, lies the one improvement of which the balloon is capable, viz. the means to secure its ascent and descent without expenditure of gas or ballast.

The power required to raise a mass which already possesses buoyancy is very slight compared with that which is requisite to propel against a resistant atmosphere. The effect also of the more simple power would be incomparably greater, because upon the supposition that a balloon required an additional power of 20lbs. beyond the gas with which it is inflated to raise it into the air, the application of a slight mechanical arrangement would clear it from all obstructions and bring it under the influence of another power, viz. the air in motion which would give it horizontal direction, and if that direction is not the one desired, it might raise it in search of another. The cessation of the mechanical action would also bring it down to the ground. Apply this power equal to 20 lbs. to a propeller working horizontally, and its inadequacy to effect any satisfactory result in a direct line becomes apparent. In comparison with the space to be travelled, the rate at which propulsion could be attained would be utterly insignificant.

Here let the imagination have a little play. For purposes of enjoyment it is not necessary to mount into the clouds. Our

aerial yacht requires but to skim the trees and buildings. Wafted across the wide expanse, it may skim the fields and rise to the hedges. With an envelope more impervious to the escape of gas, the voyage may be prolonged. Floating day after day, at the caprice of the wind certainly, but always over fresh scenery, nothing could be more enjoyable.

During these excursions it would be possible to renew the gas by proximity to some gas works, and not seldom it might happen that, by waiting for it at anchor, a favourable wind might influence a return journey. It may here be remarked that any mechanical means such as a screw acting vertically in the car of a balloon, capable of exerting a force of thirty-five pounds, would save one thousand cubic feet of gas.

It was intended by the society which I represent to try the effect of the screw acting vertically upon a balloon, but in the meantime the War Office authorities were induced to give a trial to the inventor of a screw by which he hoped effectually to propel the balloon. He was induced to attach also an arrangement which acted vertically. The following is an extract from Captain C. Orde Browne's article, published in the "Popular Science Review" for October 1874.

"The difficulty of ascertaining exactly when a captive balloon is balanced, when even a slight wind is blowing, so as to stretch the retaining rope, made the first trial a little doubtful; and after one ascent, apparently due to the working of the propeller, a doubt arose as to the exact balance of the balloon, which might have a tendency to rise, and only have been held down by the captive line, which, except at very still moments, was pulled taut by the wind acting on the balloon. It being ascertained at a still interval that the balance was good, the vertical gear was worked, and the balloon again rose. The rate of ascent was difficult to estimate; it was judged, however, not to exceed fifty feet a minute. A positive indication of the power of the propeller was thus obtained, and it should be noticed that the circumstances—if the rate of ascent only was measured—were rather disadvantageous, for the weight of the line up to the extent of forty feet was gradually added to the balloon as it rose. Had the mean rate of ascent and descent been taken, this error would be eliminated, for the descent would be favoured by the weight of the rope from forty feet in length at the maximum height, down to nothing at the ground."

The balloon was now liberated, and the report goes on to say: "The horizontal gear, however, throughout the entire voyage failed to give any satisfactory results; even allowing that the effect was perceptible, it is impossible to lay much stress upon it. Any force would give a perceptible effect if recorded with sufficient delicacy. There is no use in an insignificant effect

unless it can be shown that means exist by which it could be increased sufficiently to bear a reasonable relation to the forces to which it is to be opposed."

Therefore the experiment which the society had advocated for several years, and which it had at last determined to adopt, having proved successful under disadvantageous circumstances when tried by others, the apparatus ordered was countermanded. The opinion which had been expressed also in the various reports issued by the society as to the inutility of any screw apparatus for effecting horizontal propulsion was confirmed.

It is singular that no one has taken advantage of an ascertained fact to put the balloon to more pleasurable, because more prolonged use, than has hitherto been attempted. For instance, let us consider the mode of propulsion adopted in a punt—a clumsy kind of boat, which may often be seen moored in the Thames, in mid-stream, with harmless old gentlemen seated therein, alluring gudgeon. Well, these punts are cleverly managed with a long pole. In a rapid stream, should the punter wish to go with it, he has nothing to do but to keep it off from the bank, under the full influence of the stream; and there is every probability that with a balloon so balanced a push with a long pole would send it up spinning for fifty feet or more, and one might traverse a few hundred yards before it neared the earth and required another push. The distance traversed between each push would of course depend upon the velocity of the air current. It is evident that no ballast is necessary under such conditions, therefore the absence of that would allow of reduced size of balloon. All this, however, is simply waftage.

It is believed by some that the screw may yet serve a more useful purpose than that of the translation of a merely buoyant body. By muscular effort alone all that has been done by the power of one man has been the raising of $26\frac{1}{2}$ lbs. weight.

There has latterly been a more ambitious attempt, involving the expenditure of several hundred pounds of money.

It resulted from the experiments which the Aeronautical Society instituted with a view to record for the benefit of inventors the exact lifting pressure due to the wind advancing against a plane inclined towards it at different angles. These experiments, which took place at Messrs. Penn's factory, at Greenwich, were conducted by several well-known members of the council, and it was well understood at the time that if the results gave no encouragement for the attainment of success in utilizing the air as a highway, the society should be dissolved.

Accordingly an instrument devised by Mr. Wenham was constructed by Mr. Browning for the before-mentioned society, and submitted to a powerful blast from a fan-blower ten feet long by eighteen inches square.

The direct pressure upon one foot square of steel plate, with the blast acting at right angles to the plate, was 3·24 lbs., which, according to Rouse and Smeaton's tables, evidences a wind velocity of about twenty-five miles an hour. The same plate inclined at an angle of 15° from the horizontal, received a direct pressure of only 0·33 lbs., accompanied by a lifting pressure of 1·5 lb. There were various inclinations and different areas tried, but there is no need here to go fully into the tabulated results. It will be sufficient to say that a plane of one square foot, impelled at an angle of 15° against air moving at the rate of about twenty-five miles an hour, will support a weight of one pound and a half, whilst it will only meet with a resistance to its forward motion of five ounces and a quarter, although of course there would have to be added to this the resistance due to the form in which the weight is disposed. A less angle than 15° could not be tried owing to some obstruction in the action of the instrument, but the experiment shows very great lifting force in proportion to the power which requires to be expended in the propulsion of the plane. It shows also that the ratio of the lift to the thrust greatly increases as the inclination diminishes, whilst the force to propel is equally lessened, and thus the sustained flight of birds, often with motionless wings, is in great part accounted for. There exists also another circumstance which is favourable to the extension of the sustaining surface, viz. that the lifting power relative to the square foot increases in some yet unknown ratio with the extent of surface exposed, upon the principle which has been ascertained that the more the total area of a ship's sails is cut up into portions, the less the effect.

Such fundamental experiments accord with the legitimate duty of the society to which I have the honour and pleasure to act as honorary secretary. It is left to the spontaneous efforts of individual members to work up to the data thus established.

Upon such men, whilst the world generally look with amused pity, the eyes of a cautious and watchful few are fixed, ready to take advantage of the first hopeful result. It is my earnest hope that the society will stand between such men and injustice at the time of the general scramble for pecuniary recompense.

It will be the place here to allude to the late and very expensive attempt of Mr. Thomas Moy to construct an apparatus by means of which, in his trials in order to obtain a fulcrum upon the air, he practically tested the correctness of the facts brought out and tabulated.

Mr. Moy was an exhibitor at the Exhibition of the Society at the Crystal Palace in the year 1868. For some purpose or

another, ignoring Mr. Stringfellow's light engine, which then gained the prize, he commenced to design another which he deemed more suitable for his object. This was to actuate four driving-wheels, ten inches in diameter, to act in their turn upon two aero-plane wheels, six feet in diameter, by frictional gearing on the periphery. The aero-plane wheels had each twelve light wooden planes fitted to them, something like the screw-propeller, but with the important difference that the pitch was variable at every portion of the revolution. His theory was, that the action of these planes in their revolution through the air was a perfect mechanical imitation of the action of a bird's wing in the various positions that its surface assumes during the progress of flight, giving, as it does, an upward and forward thrust continually, without any downward force from the air on any of the aero-planes. The whole apparatus was placed upon wheels.

The steam-engine was contained in a case 27 inches by 27½ inches by 7½ inches. The diameter of its cylinder was 2⅛ inches, length of stroke 3 inches. The generating tubes contained a surface of eight square feet, and in these tubes the water circulated with very great rapidity, and thereby utilised the heat in the very best manner. There was literally no framing to the engine itself, as the driving-axle ran in a tube which passed through the steam-chest. The steam was cut off at half-stroke by the slide-valve, and with a pressure of from 120 to 160 lbs. to the square inch, and 536 revolutions per minute, it might fairly be called a 3-horse-power engine. The fuel used was methylated spirits under pressure, which produced blow-pipe flames.

With regard to the form of the so-called "aerial steamer," which was tried in March 1874, we may say that the engine was fixed about four feet from the ground, on a framework which also held the lamps. Other frames, extended from this on each side, take the axles of the 6-feet driving-wheels. These axles were 3 feet 3 inches in length and 1½ inch in diameter, made of drawn brass tube, and very light and strong. An aero-plane of 50 square feet surface was fitted in front, and one of 64 square feet behind.

These were inclined at an angle of 10° from the horizontal; and Mr. Moy calculated that if the whole could be driven on the ground at thirty-five miles an hour, it would encounter a pressure from the atmosphere sufficient to support the whole weight of the machine, which amounted with its engine, fuel, &c., to 214 lbs. The driving-surface of the revolving aero-planes was equal to 60 square feet.

Before running the "aerial steamer" round the central fountain at the Crystal Palace, Mr. Moy took steps to test the

correctness of the principle upon which he had been working, guided by the experiments undertaken by the Aeronautical Society. If the old theory was correct, he argued, the lifting pressure on the planes would only amount to a few ounces per square foot; if the new theory was correct, the pressure would far exceed that of the old. It turned out that the old theory was wrong, and the reliability of the recent experiments was confirmed. The revolving planes having been set at an angle of 15°, the pressure was found to be exactly one pound to the square foot at a speed of twenty miles an hour; and with the angle set at 45°, the pressure was 1½ lb. to each square foot. The success of the ulterior experiment, therefore, all depended upon whether he could obtain a sufficient speed upon the ground to avail himself of the lifting-pressure due to the angle of inclination.

The fountain had a path round it. The diameter of the circle was 300 feet. A pole was erected in the centre, from the top of which two cords were attached, one to each end of the machine. Though the gravel had been rolled, the action of the machine under steam was so rough and unsteady that the experiment had to be abandoned until a suitable road could be constructed.

This was eventually effected with 8,000 square feet of boarding, lent by the Crystal Palace authorities, when, after its occupation for some time by the snow, the roadway was ready for a further trial. Instead, however, of the necessary speed being attained, viz. thirty-three miles an hour, it was only possible to get about twelve, so that it was felt as a matter of regret that arrangements had not been made to run it upon a straight line of railway. The wheels, fitted for forward motion only, offered great resistance to running round a circle.

This machine, however, weighing nearly two cwt., was impelled round a circle at twelve miles an hour by the pressure of two aero-plane wheels working in the air, an achievement I believe to be quite unprecedented.

This first start from the ground has always presented great difficulties to the experimenter in aeronautics. Theory has generally favoured the incline as the readiest mode of accomplishing the object, but it is certain that unless there is power sufficient to raise the weight, safety in controlling the descent under exceptional circumstances cannot be secured, as the parachute form would be out of character in any machine designed for rapid transit through the air. In the difficulty in which Mr. Moy found himself, it was natural that he should turn to the vertical screw.

In the report of the Aeronautical Exhibition in 1868, drawn up by Mr. Wenham, the following paragraph appears:—

"Though we are still without a precise demonstration of the power required for flight in the way that a bird flies, the force to maintain which, in some species, must be very small, yet we have some evidence of the power required to lift a weight in the air by means of vertical screws. By this method it has been demonstrated that 100 pounds may be supported by a constant force of about 90,000 foot-pounds, or three horse-power.

"Now, in the work of Mr. Stringfellow, the society has brought out the remarkable fact that a one-horse power engine can be made to weigh only 13 lbs., thus showing the possibility of obtaining flight by the repudiated system of vertical screws, even with the enormous expenditure of power that this plan is known to require."

In order to ascertain what actual lifting power could be obtained with planes moving in horizontal orbits, Mr. Moy constructed new aero-plane wheels, 12 feet in diameter, with twelve planes to each wheel, the whole presenting 160 square feet of surface, driven by a steam-engine weighing 80 lbs. By placing the whole acting surface on these two wheels, an interesting experiment was carried out.

It was palpable, however, that from the conditions of the actual trial the full lifting-power due to the surface, angle, and velocity could not be hoped for. These revolving planes were travelling all the time in one circle. They had not the advantage of obtaining an abutment upon a previously undisturbed body of air. The experiment was in an enclosed part of the building. Great part of its power was expended in drawing downwards a body of air. The whole weight of the machine was 186 lbs. Levers were attached to the spindle of the aero-plane wheels, which were weighted to take off all over 120 lbs. This latter weight was raised from the floor—according to the independent testimony of Captain Greenfield, of the Royal Artillery—as much as six inches under one aero-plane, and two inches under the other, this inequality being due to one wing-plane having broken.

The engine, therefore, was proved capable of raising itself, and 40 lbs. additional weight, under great disadvantages. The revolutions of these two 12-feet aero-planes were sixty-seven per minute.

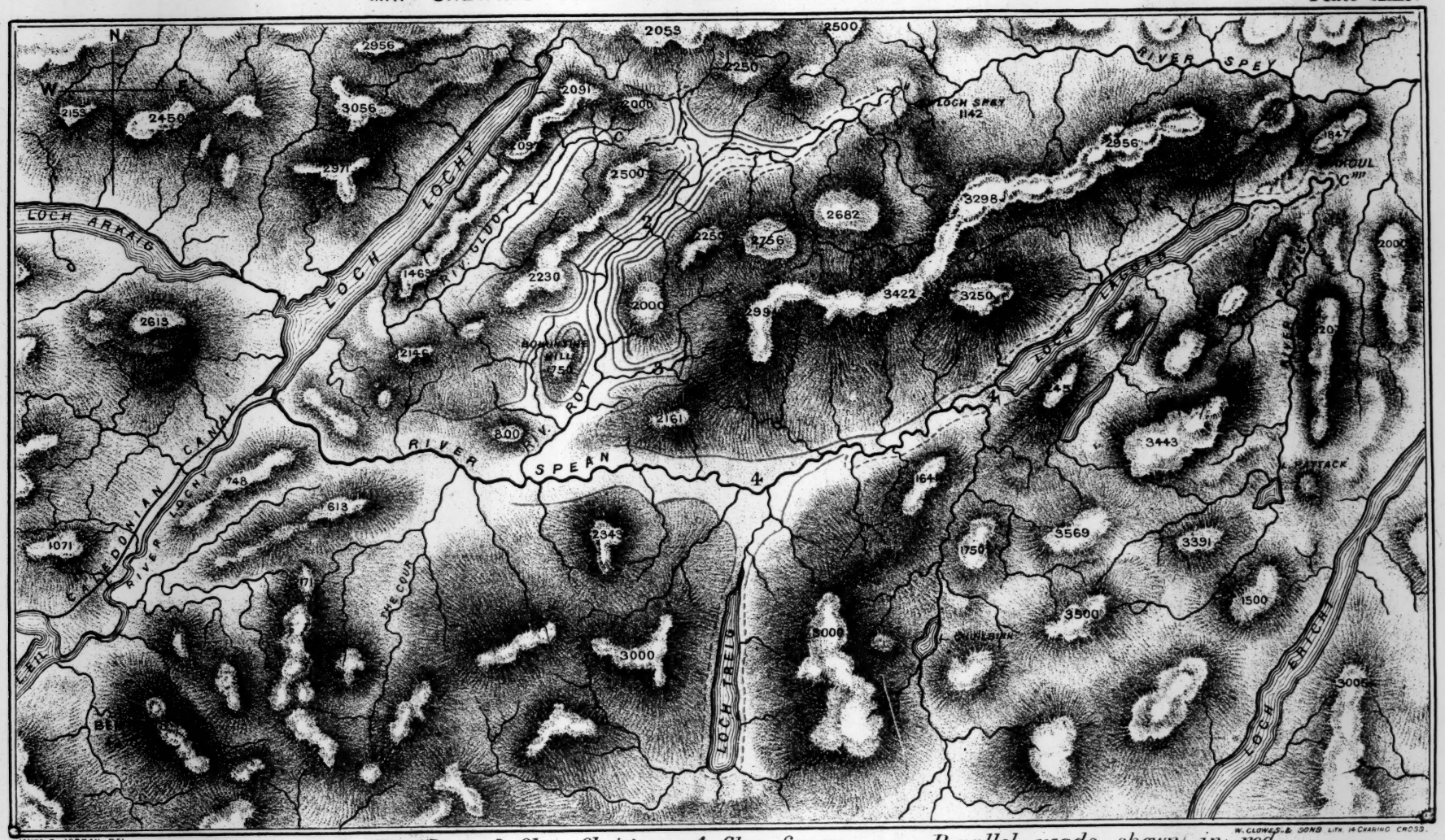
The preparations for the experiments, which have here so easily been summed up, occupied winters and summers. Repeated breakages, renewals, strengthenings, re-construction, re-adjustment, both in engine and apparatus, testify to the patient perseverance of the inventor, and those associated with him. And though lastly mentioned, yet by no means the least, was the constant leakage of the not too auriferous money-bag.

This, however, is the common lot. The situation can only be estimated by the spectator—such as I am—of many abortive attempts.

My space is consumed before my pen has run dry, else I might adduce some more reasons why such earnest workers in the solution of the profoundest problem which ever absorbed the brain-power of aspiring man should be encouraged by the wealthy to go on and progress in Aeronautics.

MAP SHEWING THE PARALLEL ROADS OF GLEN ROY.

Plate CXLI.



JAMES B. JORDAN, DEL.

1. Glen Gluoy. 2. Glen Roy – 3. Glen Glaister – 4. Glen Spean *Parallel roads shewn in red*

Heights marked in feet 0 1 2 3 4 5 6 7 8 9 10 Scale of Miles.

W. CLOWES & SONS LITH. 14 CHARING CROSS.

THE PARALLEL ROADS OF GLEN ROY.

By Professor TYNDALL, D.C.L., LL.D., F.R.S., &c.

[PLATE CXLI.]

THE first published allusion to the Parallel Roads of Glen Roy occurs in the appendix to the third volume of Pennant's "Tour in Scotland," a work published in 1776. "In the face of these hills," says this writer, "both sides of the glen, there are three roads at small distances from each other and directly opposite on each side. These roads have been measured in the complete parts of them, and found to be 26 paces of a man 5 feet 10 inches high. The two highest are pretty near each other, about 50 yards, and the lowest double that distance from the nearest to it. They are carried along the sides of the glen with the utmost regularity, nearly as exact as drawn with a line of rule and compass."

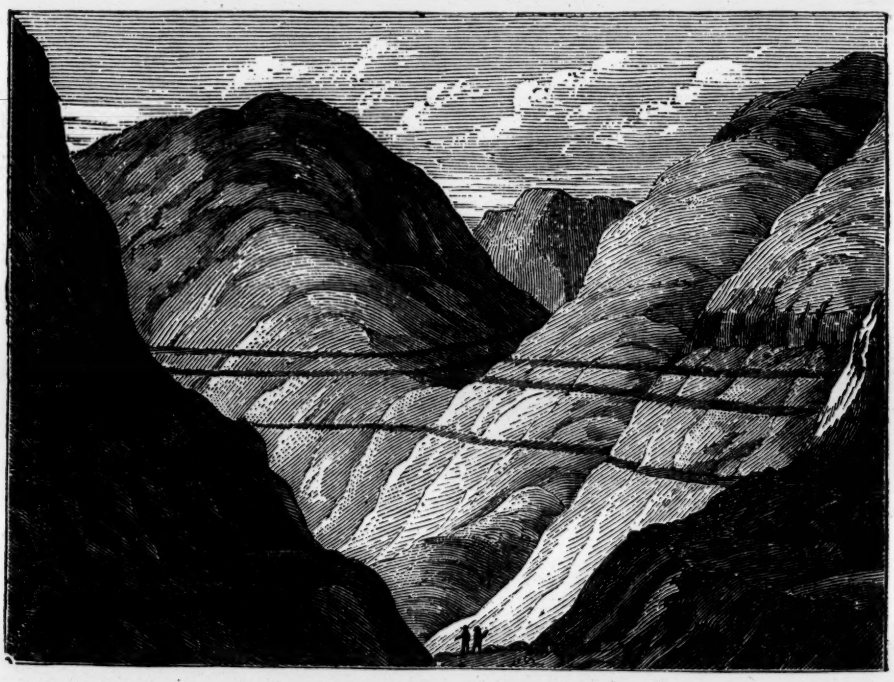
The correct heights of the three roads of Glen Roy are respectively 1,150, 1,070, and 860 above the sea. Hence a vertical distance of 80 feet separates the two highest, while the lowest road is 210 feet below the middle one.

These "roads" are usually shelves or terraces formed in the yielding drift which here covers the slopes of the mountains. They are all sensibly horizontal, and therefore parallel. Pennant accepted as reasonable the explanation of them given by the country people, who thought "they were designed for the chase, and that the terraces were made after the spots were cleared in lines from wood, in order to tempt the animals into the open paths after they were rouzed in order that they might come within reach of the bowmen who might conceal themselves in the woods above and below."

In these attempts of "the country people" we have an illustration of that impulse to which all scientific knowledge is due—the desire to know the causes of things; and it is a matter of surprise that in the case of the parallel roads, with their weird appearance challenging inquiry, this impulse did

not make itself more rapidly and energetically felt. Their remoteness may perhaps account for the fact that until the year 1817 no systematic description of them, and no scientific attempt at an explanation of them, appeared. In that year Dr. MacCulloch, who was then President of the Geological Society, presented to that Society a memoir, in which the roads were discussed, and regarded as the margins of lakes once embosomed in Glen Roy.

To Dr. MacCulloch succeeded a man, possibly not so learned as a geologist, but obviously fitted by nature to grapple with her facts and to put them in their proper setting. I refer to



PARALLEL ROADS OF GLEN ROY.
After a Sketch by Sir Thomas Dick-Lauder.

Sir Thomas Dick-Lauder, who presented to the Royal Society of Edinburgh, on the 2nd of March, 1818, his paper on the Parallel Roads of Glen Roy. In looking over the literature of this subject, which is now copious, it is interesting to observe the differentiation of minds, and to single out those who went by a kind of instinct to the core of the question, from those who erred in it, or who learnedly occupied themselves with its analogies, adjuncts, and details. There is no man, in my opinion, connected with the history of the subject, who has shown, in relation to it, this spirit of penetration, this force of scientific insight, more conspicuously than Sir Thomas Dick-Lauder. Two distinct mental processes are involved in its

treatment. Firstly, the faithful and sufficient observation of the data; and, secondly, that higher mental process in which the constructive imagination comes into play, connecting the separate facts of observation with their common cause, and weaving them into an organic whole. In neither of these requirements did Sir Thomas Dick-Lauder fail.

Adjacent to Glen Roy is a valley called Glen Gluoy, along the sides of which ran a single shelf, or terrace, formed obviously in the same manner as the parallel roads of Glen Roy. The two shelves on the opposing sides of the glen were at precisely the same level, and Dick-Lauder wished to see whether, and how, they became united at the head of the glen. He followed the shelves into the recesses of the mountains. The bottom of the valley, as it rose, came ever nearer to them, until finally, at the head of Glen Gluoy, he reached a col, or watershed, of precisely the same elevation as the road which swept round the glen.

The correct height of this col is 1,170 feet above the sea. It is therefore 20 feet above the highest road in Glen Roy.

From this col a lateral branch-valley led towards Glen Roy. Our explorer descended from the col to the highest road in that glen, and pursued it exactly as he had pursued the road in Glen Gluoy. For a time it belted the mountain sides at a considerable height above the bottom of the valley; but this rose as he proceeded, coming ever nearer to the highest shelf, until finally he reached a col, or watershed, looking into Glen Spey, and of precisely the same elevation as the highest parallel road of Glen Roy.

He then dropped down to the lowest of these roads, and followed it towards the mouth of the glen. Its elevation above the bottom of the valley gradually increased; not because it rose, but because it remained level while the valley sloped downwards. He found this lowest road doubling round the hills at the mouth of Glen Roy, and running along the sides of the mountains which flank Glen Spean. He followed it eastward. The Spean Valley, like the others, gradually rose, and therefore gradually approached the road on the adjacent mountain-side. He came to Loch Laggan, the surface of which rose almost to the level of the road, and beyond the head of this lake he found, as in the other two cases, a col, or watershed, of exactly the same level as the single road in Glen Spean, which, it will be remembered, is a continuation of the lowest road in Glen Roy.

Here we have a series of facts of obvious significance as regards the solution of this question. The effort of the mind to form a coherent image from such facts, might be compared with the effort of the eyes to cause the pictures of the stereo-

scope to coalesce. For a time we exercise a certain strain, the object remaining vague and indistinct. Suddenly its various parts seem to run together, the object starting forth in clear and definite relief. Such, I take it, was the effect of his ponderings upon the mind of Sir Thomas Dick-Lauder. His solution was this: Taking all their features into account, he was convinced that water only could have produced the terraces. He saw clearly that, supposing the mouth of Glen Gluoy to be stopped by a barrier, if the water from the mountains flanking the glen were allowed to collect, it would form behind the barrier a lake, the surface of which would gradually rise until it reached the level of the col at the head of the glen. The rising would then cease; the superfluous water of Glen Gluoy discharging itself over the col into Glen Roy. As long as the barrier stopping the mouth of Glen Gluoy continued, we should have in that glen a lake at the precise level of its shelf, which lake, acting upon the loose drift of the flanking mountains, would actually form the shelf revealed by observation.

So much for Glen Gluoy. But suppose the mouth of Glen Roy also stopped by a barrier sufficiently high. Behind it, the water from the adjacent mountains would collect. The surface of the lake thus formed would gradually rise, until it had reached the level of the col which divides Glen Roy from Glen Spey. Here the rising of the lake would cease; its superabundant water being poured over the col into the valley of the Spey. This state of things would continue as long as the barrier remained at the mouth of Glen Roy. The lake thus dammed in, with its surface at the level of the highest parallel road, would act, as in Glen Gluoy, upon the friable drift overspreading the mountains, and would form the highest road or terrace of Glen Roy.

And now let us suppose the barrier to be so far removed from the mouth of Glen Roy as to establish a connection between it and the upper part of Glen Spean, while the lower part of the latter glen continued blocked up. Upper Glen Spean and Glen Roy would then be occupied by a continuous lake, the level of which would obviously be determined by the col at the head of Loch Laggan. The water in Glen Roy would sink from the level it had previously maintained, to the level of its new place of escape. This new lake-surface would correspond exactly with the lowest parallel road, and it would form that road by its action upon the drift of the adjacent mountains.

In presence of the observed facts, this solution commends itself strongly to the scientific mind. The question next occurs, What was the character of the assumed barrier which stopped the glens? There are at the present moment vast masses of

detritus in certain portions of Glen Spean, and of such detritus Sir Thomas Dick-Lauder imagined his barriers to have been formed. By some unknown convulsion, this detritus had been heaped up. But, once given, and once granted that it was subsequently removed, the single road of Glen Gluoy and the highest and lowest roads of Glen Roy would be explained in a satisfactory manner.

To account for the second or middle road of Glen Roy, Sir Thomas Dick-Lauder invoked a new agency. He supposed that at a certain point in the breaking down or waste of his dam, a halt occurred, the barrier holding its ground at a particular level sufficiently long to dam a lake rising to the height of, and forming the second road. This point of weakness was at once detected by Mr. Darwin, and adduced by him as proving that the levels of the cols did not constitute an essential feature in the phenomena of the parallel roads. Though not destroyed, Sir Thomas Dick-Lauder's theory was seriously shaken by this argument, and it became a point of capital importance, if the facts permitted, to remove such source of weakness. This was done in 1847 by Mr. David Milne, now Mr. Milne-Home. On walking up Glen Roy from Roy Bridge, we pass the mouth of a lateral glen, called Glen Glaster, running eastward from Glen Roy. There is nothing in this lateral glen to attract attention, or to suggest that it could have any conspicuous influence in the production of the parallel roads. Hence, I think, the failure of Sir Thomas Dick-Lauder to notice it. But Mr. Milne-Home entered this glen, on the northern side of which the middle and lowest roads are fairly shown. The principal stream running through the glen turns at a certain point northwards and loses itself among hills too high to offer any outlet. But another branch of the glen turns to the south-east; and, following up this branch, Mr. Milne-Home reached a col, or watershed, of the precise level of the second Glen Roy Road. When the barrier blocking the glens had been so far removed as to open this col, the water in Glen Roy would sink to the level of the second road. A new lake of diminished depth would be thus formed, the surplus water of which would escape over the Glen Glaster col into Glen Spean. The margin of this new lake, acting upon the detrital matter, would form the second road. The theory of Sir Thomas Dick-Lauder, as regards the part played by the cols, was re-rivetted by this new and unexpected discovery.

I have referred to Mr. Darwin, whose powerful mind swayed for a time the convictions of the scientific world in relation to this question. His notion was—and it is a notion which very naturally presents itself—that the parallel roads were formed by the sea; that this whole region was once submerged and

subsequently upheaved; that there were pauses in the proces upheaval, during which these glens constituted so many fio on the sides of which the parallel terraces were formed. T theory will not bear close criticism; nor is it now maintai by Mr. Darwin himself. It would not account for the sea be 20 feet higher in Glen Gluoy than in Glen Roy. It would account for the absence of the second and third Glen Roy ro from Glen Gluoy, where the mountain flanks are quite impressionable as in Glen Roy. It would not account for absence of the shelves from the other mountains in the ne bourhood, all of which would have been clasped by the sea the sea been there. Here then, and no doubt elsewhere, Darwin has shown himself to be fallible; but here, as elsewh he has shown himself equal to that discipline of surrende evidence which girds his intellect with unassailable m strength.

But, granting the significance of Sir Thomas Dick-Laud facts, and the reasonableness, on the whole, of the views wh he has founded on them, they will not bear examinatio detail. No such barriers of detritus as he assumed could h existed without leaving traces behind them; but there is trace left. There is detritus enough in Glen Spean, but where it is wanted. The two highest parallel roads abruptly at different points near the mouth of Glen Roy, bu remnant of the barrier against which they abutted is to be s It might be urged that the subsequent invasion of the valley glaciers has swept the detritus away; but there have beer glaciers in these valleys since the retreat of the lakes. Profe Geike has favoured me with a drawing of the Glen Spean s near the entrance to Glen Trieg. The shelf forms a belt ro a great mound of detritus which, had a glacier followed the mation of the shelf, must have been cleared away. Taking the circumstances into account, you may, I think, with sa dismiss the detrital barrier as incompetent to account for present condition of Glen Gluoy and Glen Roy.

Hypotheses in science, though apparently transcending exp ence, are in reality experience modified by scientific thou and pushed into an ultra experiential region. At the time t he wrote, Sir Thomas Dick-Lauder could not possibly h assigned the cause subsequently assigned for the blockag these glens. A knowledge of the action of ancient glaciers the necessary antecedent to the new explanation, and experie of this nature was not possessed by the distinguished writer mentioned. The extension of Swiss glaciers far beyond t present limits was first made known by a Swiss engin named Venetz, who established, by the marks they had behind, their former existence in places which they had l

forsaken. The subject of glacier extension was subsequent followed up with distinguished success by Charpentier, Stud and others. Agassiz grappled with it with characteristic vigou extending his evidences far beyond the domain of Switzerlan He came to this country in 1840, and found in various plac indubitable marks of ancient glacier action. England, Scotlan Wales, and Ireland he proved to have once given birth glaciers. He visited Glen Roy, surveyed the surroundi neighbourhood, and pronounced, as a consequence of his inves gation, the barriers which stopped the glens and produced t parallel roads to have been barriers of ice. To Mr. Jamiesc above all others, we are indebted for the thorough testing a confirmation of this theory.

And let me here say that Agassiz is only too likely to misrated and misjudged by those who fail to grasp in th totality the motive powers invoked in scientific research. lacked mechanical precision, but he abounded in that force a freshness of the scientific imagination which in some scienc and probably in some stages of all sciences, are essential to t creator of knowledge. To Agassiz was given, not the art of t refiner, but the instinct of the discoverer, and the strength the delver who brings ore from the recesses of the mine. Th ore may contain its share of dross, but it also contains t precious metal which gives employment to the refiner, a without which his occupation would depart.

Let us dwell for a moment upon this subject of ancie glaciers. Under a flask containing water, in which a therm meter is immersed, is placed a Bunsen's lamp. The water heated, reaches a temperature of 212°, and then begins to bc The rise of the thermometer then ceases, although heat co tinues to be poured by the lamp into the water. What becom of that heat? We know that it is consumed in the molecul work of vaporization. In the experiment here arranged, t steam passes from the flask through a tube into a second ves kept at a low temperature. Here it is condensed, and inde congealed to ice, the second vessel being plunged in a mixtu cold enough to freeze the water. As a result of the process obtain a mass of ice. That ice has an origin very antitheti to its own character. Though cold, it is the child of heat. we removed the Bunsen lamp, there would be no steam, and there were no steam there would be no ice. The mere cold the mixture surrounding the second vessel would not produ ice. The cold must have the proper material to work upor and this material—aqueous vapour—is, as we here see, t direct product of heat.

It is now, I suppose, fifteen or sixteen years since I fou myself conversing with an illustrious philosopher regarding th

glacial epoch which the researches of Agassiz and others had revealed. This profoundly thoughtful man was of opinion that, at a certain stage in the history of the solar system, the sun's radiation had suffered diminution, the glacial epoch being a consequence of this central chill. The celebrated French mathematician Poisson had another theory. Astronomers have shown that the solar system moves through space, and the temperature of space is a familiar conception with scientific men. It was considered probable by Poisson that our system, during its motion, had traversed portions of space of different temperatures; and that during its passage through one of the colder regions of the universe, the glacial epoch occurred. Notions such as these were more or less current not many years ago, and I therefore thought it worth while to show how incomplete they were. Suppose the temperature of our planet to be reduced, by the subsidence of solar heat, the cold of space, or any other cause, say one hundred degrees. Four-and-twenty hours of such a chill would bring down as snow nearly all the moisture of our atmosphere. But this would not produce a glacial epoch. Such an epoch would require the continuous generation of the material from which the ice of glaciers is derived. Mountain snow, the nutriment of glaciers, is derived from aqueous vapour raised mainly from the tropical ocean by the sun. The solar fire is as necessary a factor in the process as our Bunsen lamp in the experiment referred to a moment ago. Nothing is easier than to calculate the exact amount of heat expended by the sun in the production of a glacier. It would, as I have elsewhere shown,* raise a quantity of cast-iron five times the weight of the glacier not only to a white heat, but to its point of fusion. If, as I have urged elsewhere, instead of being filled with ice, the valleys of the Alps were filled with white-hot metal, of quintuple the mass of the present glaciers, it is the heat, and not the cold, that would arrest our attention and solicit our explanation. The process of glacier-making is obviously one of distillation, in which the fire of the sun which generates the vapour plays as essential a part as the cold of the mountains which condenses it.†

It was their ascription to glacier action that first gave the parallel roads of Glen Roy an interest in my eyes; and in 1867,

* "Heat a Mode of Motion," fifth edition, chap. vi.; Forms of Water, §§ 55 and 56.

† In Lyell's excellent "Principles of Geology," the remark occurs that "several writers have fallen into the strange error of supposing that the glacial period must have been one of higher mean temperature than usual." The really strange error was the forgetfulness of the fact that, in the production of glaciers, heat played quite as important a part as cold.

with a view to self-instruction, I made a solitary pilgrimage to the place, and explored pretty thoroughly the roads of the principal glen. I traced the highest road to the col dividing Glen Roy from Glen Spey, and, thanks to the civility of an Ordnance surveyor, I was enabled to inspect some of the roads with a theodolite. As stated by Pennant, the width of the roads amounts sometimes to more than twenty yards; but near the head of Glen Roy the highest road ceases to have any width, for it runs along the face of a rock, the effect of the lapping of the water on the more friable portions of the rock being perfectly distinct to this hour. My knowledge of the region was, however, far from complete, and nine years had dimmed the memory even of the portion which I had thoroughly examined. Hence my desire to see the roads once more before venturing to talk to you about them. The Easter holidays were to be devoted to this purpose; but at the last moment a telegram from Roy Bridge informed me that the roads were snowed up. I was thus thrown back upon books and memories; but these proving only a poor substitute for the flavour of facts, I resolved subsequently to make another effort to see the roads. Accordingly on Thursday fortnight, after lecturing at the Royal Institution, I packed up, and started (not this time alone) for the North. Next day at noon we found ourselves at Dalwhinnie, whence a drive of some five-and-thirty miles brought us to the excellent hostelry of Mr. Macintosh, at the mouth of Glen Roy.

We might have found the hills covered with mist, which would have wholly defeated us; but Nature was good-natured, and we had two successful working days among the hills. Guided by the excellent Ordnance map, on the Saturday morning we went up the glen, and, on reaching the stream called Allt Bhreac Achaidh, faced the hills to the west. At the watershed between Glen Roy and Glen Fintaig we bore northwards, struck the ridge above Glen Gluoy, came in view of its road, which we persistently followed as long as it continued visible. It is a feature of all the roads that they vanish before reaching the cols over which fell the waters of the lakes which formed them. One reason doubtless is that at their upper ends the lakes were shallow, and incompetent on this account to raise wavelets of any strength to act upon the mountain drift. A second reason is that they were land-locked in the higher portions and protected from the south-westerly winds, the stillness of their waters causing them to produce but a feeble impression upon the mountain sides. From Glen Gluoy we passed down Glen Turrit to Glen Roy, and through it homewards, thus accomplishing two or three-and-twenty miles of rough and honest work.

Next day we thoroughly explored Glen Glaster, following its

two roads as far as they were visible. We reached the col discovered by Mr. Milne-Home, and which stands at the level of the middle road of Glen Roy. Thence we crossed southwards over the mountain *Creag Dhubh*, and examined the erratic blocks upon its sides, and the ridges and mounds of moraine matter which cumber the lower flanks of the mountain. The observations of Mr. Jamieson upon this region, including the mouth of Glen Trieg, are in the highest degree interesting. We entered Glen Spean, and continued a search begun on the evening of our arrival at Roy Bridge—the search, namely, for glacier polishings and markings. We did not find them copious, but they are indubitable. One of the proofs most convenient for reference is a great rounded rock by the road side, 1,000 yards east of the milestone marked three-quarters of a mile from Roy Bridge. Farther east other cases occur, and they leave no doubt upon the mind that Glen Spean was at one time filled by a great glacier. To the disciplined eye the aspect of the mountains is perfectly conclusive on this point; and in no position can the observer more readily and thoroughly convince himself of this than at the head of Glen Glaster. The dominant hills here are all intensely glaciated.

But the great collecting ground of the glaciers which dammed the glens and produced the parallel roads were the mountains south and west of Glen Spean. The monarch of these is Ben Nevis, 4,370 feet high. The position of Ben Nevis and his colleagues, in reference to the vapour-laden winds of the Atlantic, is a point of the first importance. It is exactly similar to that of Carrantual and the Macgillicuddy Reeks in the south-west of Ireland. These mountains are, and were, the first to encounter the south-western Atlantic winds, and the precipitation, even at present, in the neighbourhood of Killarney, is enormous. The winds, robbed of their vapour, and charged with the heat set free by its precipitation, pursue their direction obliquely across Ireland; and the effect of the drying process may be understood by comparing the rainfall at Cahirciveen with that at Portarlington. As found by Dr. Lloyd, the ratio is as 59 to 21—fifty-nine inches annually at Cahirciveen to twenty-one at Portarlington. During the glacial epoch this vapour fell as snow, and the consequence was a system of glaciers which have left traces and evidences of the most impressive character in the region of the Killarney Lakes. I have referred in other places to the great glacier which, descending from the Reeks, moved through the Black Valley, took possession of the lake-basins, and left its traces on every rock and island emergent from the waters of the upper lake. They are all conspicuously glaciated. Not in Switzerland itself do we find clearer traces of ancient glacier action.

What the Macgillicuddy Reeks did in Ireland, Ben Nevis and the adjacent mountains did, and continue to do, in Scotland. We had an example of this on the morning we quitted Roy Bridge. From the bridge westward rain fell copiously, and the roads were wet; but the precipitation ceased near Loch Laggan, whence eastward the roads were dry. Measured by the gauge, the rainfall at Fort William is 86 inches, while at Laggan it is only 46 inches annually. The difference between west and east is forcibly brought out by observations at the two ends of the Caledonian Canal. Fort William at the south-western end has, as just stated, 86 inches, while Culloden, at its north-east end, has only 24. To the researches of that able and accomplished meteorologist, Mr. Buchan, we are indebted for these and other data of the most interesting and valuable kind.

Adhering to the facts now presented to us, it is not difficult to restore in idea the process by which the glaciers of Lochaber were produced and the glens dammed by ice. When the cold of the glacial epoch began to invade the Scottish hills, the sun at the same time acting with sufficient power upon the tropical ocean, the vapours raised and drifted on to these northern mountains were more and more converted into snow. This slid down the slopes, and from every valley, strath, and corry south of Glen Spean, glaciers were poured into that glen. The two great factors here brought into play are the nutrition of the glaciers by the frozen material above, and their consumption in the milder air below. For a period supply exceeded consumption, and the ice extended, filling Glen Spean to an ever-increasing height, and abutting against the mountains to the north of that glen. But why, it may be asked, should the valleys south of Glen Spean be receptacles of ice at a time when those north of it were receptacles of water? The answer is to be found in the position and the greater elevation of the mountains south of Glen Spean. They first received the loads of moisture carried by the Atlantic winds, and not until they had been in part dried, and warmed by the liberation of their latent heat, did these winds touch the hills north of the Glen.

An instructive observation bearing upon this point is here to be noted. Had our visit been in the winter we should have found all the mountains covered; had it been in the summer we should have found the snow all gone. But happily it was at a season when the aspect of the mountains north and south of Glen Spean exhibited their relative powers as snow-collectors. Scanning the former hills from many points of view, we were hardly able to detect a fleck of snow, while heavy swaths and patches loaded the latter. Were the glacial epoch to return, the relation indicated by this observation would cause Glen Spean to be filled with glaciers from the south, while the hills and

valleys on the north, visited by milder and drier winds, would remain comparatively free from ice. This flow from the south would be reinforced from the west, and as long as the supply was in excess of the consumption the glaciers would extend, the dams closing the glens increasing in height. By-and-by supply and consumption becoming approximately equal, the height of the glacier barriers would remain constant. Then, as milder weather set in, consumption would be in excess, and a retreat of the ice would be the consequence. But for a long time the conflict between supply and consumption would continue, retarding indefinitely the disappearance of the barriers, and keeping the imprisoned lakes in the northern glens. But however slow its retreat, the ice in the long run would be forced to yield. The dam at the mouth of Glen Roy, which probably entered the glen sufficiently far to block up Glen Glaster, would gradually retreat. Glen Glaster and its col being opened, the subsidence of the lake 80 feet, from the level of the highest to that of the second parallel road, would follow as a consequence. I think this the most probable course of things; but it is also possible that Glen Glaster may have been blocked by a glacier from Glen Trieg. The ice dam continuing to retreat, at length permitted Glen Roy to connect itself with upper Glen Spean. A continuous lake then filled both glens, the level of which, as already explained, was determined by the col at Makul, above the head of Loch Laggan. The last to yield was the portion of the glacier which derived nutrition from Ben Nevis, and probably also from the mountains north and south of Loch Arkaig. But it at length yielded, and the waters in the glens resumed the courses which they pursue to-day.

For the removal of the ice barriers no cataclysm is to be invoked; the gradual melting of the dam would produce the entire series of phenomena. In sinking from col to col the water would flow over a melting barrier, the surface of the imprisoned lakes not remaining sufficiently long at any particular level to produce a shelf comparable to the parallel roads. By temporary halts in the process of melting due to atmospheric conditions or to the character of the dam itself, or through local softness in the drift, small pseudo-terraces would be formed which, to the perplexity of some observers, are seen upon the flanks of the glens to-day.

In presence, then, of the fact that the barriers which stopped these glens to a height, it may be, of 1,500 feet above the bottom of Glen Spean, have dissolved and left not a wreck behind; in presence of the fact, insisted on by Professor Geikie, that barriers of detritus would undoubtedly have been able to maintain themselves had they ever been there; in presence of the fact that great glaciers once most certainly filled these

valleys—that the whole region, as proved by Mr. Jamieson, is filled with the traces of their action; the theory which ascribes the parallel roads to lakes dammed by barriers of ice has, in my opinion, an amount of probability on its side which amounts to a practical demonstration of its truth.

Into the details of the terrace formation I do not enter. Mr. Darwin and Mr. Jamieson on the one side, and Sir John Lubbock on the other, deal with true causes. The terraces, no doubt, are due in part to the descending drift arrested by the water, and in part to the fretting of the wavelets, and the rearrangement of the stirred detritus, along the belts of contact of lake and hill. The descent of matter must have been frequent when the drift was unbound by the rootlets which hold it together now. In some cases, it may be remarked, the visibility of the roads is materially exalted by differences of vegetation. The grass upon the terraces is not always of the same character as that above and below them, while on heather-covered hills the absence of the dark shrub from the roads greatly enhances their conspicuousness.

Reviewing our work, we find three considerable steps to have marked the solution of the problem of the Parallel Roads of Glen Roy. The first of these was taken by Sir Thomas Dick-Lauder, the second was the pregnant conception of Agassiz regarding glacier action, and the third was the testing and verification of this conception by the very thorough researches of Mr. Jamieson.* To these may be added the important observation of Mr. Milne-Home in Glen Glaster; with other remarks and reflections scattered through the literature of the subject, or suggested by the latest visit to the spot.

Thus ends our rapid survey of this brief episode in the physical history of the Scottish hills—brief, that is to say, in comparison with the immeasurable lapses of time through which, to produce its varied structure and appearances, our planet must have passed. In the survey of such a field two things are specially worthy to be taken into account—the widening of the intellectual horizon and the reaction of expanding knowledge upon the

* No circumstance, or incident, connected with this discourse gives me greater pleasure than the recognition of the value of these researches. They are marked throughout by unflagging industry, by novelty and acuteness of observation, and by reasoning power of a high and varied kind. These pages had been returned "for press" when I learned that the relation of Ben Nevis and his colleagues to the vapour-laden winds of the Atlantic had not escaped Mr. Jamieson. To him obviously the exploration of Lochaber, and the development of the theory of the Parallel Roads, has been a labour of love.

intellectual organ itself. At first, as in the case of ancient glaciers, through sheer want of capacity, the mind refuses to take in revealed facts. But by degrees the steady contemplation of these facts so strengthens and expands the intellectual powers, that where truth once could not find an entrance it eventually finds a home.—*A Lecture delivered before the Royal Institution, June* 9, 1876.

WHAT IS THE MEANING OF HUMAN PERSONALITY?

By HENRY J. SLACK, F.G.S., Sec. R.M.S.

WHAT constitutes human personality? It is a kind of consciousness, complex in character. Memory is concerned in it, or we should not associate the past sufficiently with the present, or with the closely recent, to supply the means of forming the conception of continuity. A mere reproduction by memory of the image of the past would not be sufficient to aid us in this matter; for, as Dr. Carpenter observes, "there must be a recognition of the reproduced state of consciousness as one which has been formerly experienced; and this involves a distinct mental state, which has been termed the 'consciousness of agreement.' Without this recognition we should live in the present alone."

Thus a consciousness of existing *now* combined with this "consciousness of agreement," as relates to the past conditions which memory recalls, are necessary to the idea, or sensation of personality, as it exists in man unimpaired by disease. A lower kind of personality might comprehend present existence only; but if a creature so constituted were able to profit by the results of experience, it would be as automatically as if it were a machine constructed to modify its action after receiving certain impulses. It would not be capable of human *experience*, that is of experience in the sense in which the word is usually employed to designate an impression that was once a subject of consciousness and its conscious recollection.

Dugald Stewart remarked that "we cannot properly be said to be conscious of our existence, our knowledge of this fact being necessarily posterior in the order of time to the consciousness of those sensations by which it is suggested." The time occupied in the transmission of an impression from, say a finger, with its sense of touch, to the sensorium, and its conversion into a sensation of resistance, hardness, softness, smoothness, or roughness, is a minute fraction of a second. Nerve action, like

electrical action, appears to consist in pulsations, and the *personality* of the supposed simpler creature with the lower personality would consist in intermittent productions of consciousness. But, if personality is a matter of consciousness, and is thus distinguished from the mere individual or distinct existence belonging to minerals, such a being would be, and not be, a person, in alternations, like the beats of a clock. The unconscious intervals might be long or short; the sensation would be the same. It is conceivable that beings might exist whose pulsations of consciousness and personality varied from millionths of a second to millions of years. If able to think, and satisfied with Descartes' maxim, *cogito ergo sum*—"I think therefore I am"—one such would be certified of its existence through either alternating millionths of a second or millions of years.

Dr. Carpenter's view of personality involves a series of nervous actions, each of which may be regarded as pulsations; and we have no reason to affirm that the rate of our nerve action, or pulsation, is the only one that can answer the purpose of producing a sense of personality bearing analogy to our own. We set our clocks by the beats of seconds; but if we lived on the sun, and had sight of the central sun round which he is supposed to move and carry his attendant worlds in millions of years, we might substitute centuries for seconds as our time units, and perhaps could do well with nerve pulsations proportionably long in their intermittent intervals. At each beat we should be persons, in the intervals no persons; but memory and consciousness would tie together the periods of personality, and those of no personality could be known only as inferences which knowledge might enable us to draw.

If personality, such as Dr. Carpenter defines it, could exist in all the stages of insect life—egg, grub, chrysalis, butterfly—the sense of continuity would be handed on through quite different states of existence. In the case of man, his healthiest normal personality runs through periods in which he develops, or changes, within limits that are much narrower, and which cause him to retain one character, or nature, throughout. In many cases of insanity, however, the change of character is so great that the afflicted person may be said to have become some one else. "A man beside himself" is a well-known phrase, intimating the sort of dual being that passion, or disease, or narcotics, may make of what should be an orderly uniform being.

The perceptive, the intellectual, and the moral faculties, are all subject to modification from cerebral disturbance, or disease. In some cases the sufferer is conscious of his errors, or delusions; and a well-known physician, having the care of the insane, informs the writer that hopes and prospects of cure are then

much stronger than when the patient cannot be persuaded there is anything wrong with him. In these instances there is a kind of double personality, one surveying the other, and knowing it to be wrong-headed. When the errors, or delusions, are not recognised, the personality is single, but different from that which existed in a sound state. When insane people of normally good characters fancy they have committed crimes, and feel horror and remorse, the character, or personality, remains the same, but is the victim of delusion. From this mode of speaking it must not be supposed that character and personality are regarded as the *same* thing; but what is called *character* consists in attributes of the personality, and when those attributes suffer a great change, the result is like a transformation of one person into another. Thus, in a case mentioned by Forbes Winslow, on the authority of Dr. Brierre de Boismont, a person in high office, who had performed the duties of his station satisfactorily, and in private life exhibited generosity and honesty, became mean, avaricious, licentious, and fraudulent. Similar disorders cause "the brave and heroic to become as timid and bashful as any maiden in particular states of ill health. Mild, inoffensive, and humane men are driven to acts of desperation and cruelty;" and modest girls indulge in indelicate actions and disgusting talk. The late Forbes Winslow thought such outbursts of evil came from the "innate corruption and natural depravity of the human heart"; but scientific men do not allow theological crotchets to serve as explanations for physical facts; and although no one can afford the slightest explanation of why and how thought and emotion are connected with chemical and molecular changes in nervous or cerebral matter, abundant cases prove that physical disturbance by disease or wounds can produce the changes of character which we are now considering. Not only can mechanical violence change character in certain cases for the worse, but we find opposite instances recorded where there has been a beneficial result. Thus, to cite an instance from Forbes Winslow's unphilosophical but amusing and useful story-book, "Obscure Diseases of the Brain and Mind,"—"a child, up to the age of thirteen idiotic, giving evidence either of a total deficiency of intelligence or of a stunted intellect of the lowest grade and order, fell from a height upon his head, and was stunned. He rallied from this state of unconsciousness, and was, *credat Judæus*! found to be in full possession of his intellectual faculties." Father Mabillon is also said to have been cured of idiocy, at the age of twenty-six, by tumbling against a stone staircase and fracturing his skull, for which he was trepanned, and thereupon exhibited a "lively imagination, an amazing memory, and a zeal for study unequalled." In another instance

a soldier's intellect was improved by a musket-ball knocking out some of his brains.

In the two cases just mentioned, the personality of the boy and that of the priest was changed from an idiotic one to a rational one by a physical accident; and it is interesting to compare such instances with those of the opposite sort, where, instead of the new and induced character being better, it has been horribly worse.

When persons were observed to suffer under that kind of dualism in which their proper selves were tempted and tormented by the second and morbid self, we cannot wonder that our forefathers, ignorant of science, thought them subjects of demoniacal possession. There seemed to be two distinct persons in one body; and many of the old ascetics, who, like modern lunatics, knocked their heads against stones, and tormented their flesh, acted under the notion that their real personality—that which they thought themselves—had to fight with a hostile and diabolical personality which occupied and used their bodies. When the delusions are constant, and the original man struggles against them, the dual personality is complete and continuous; but cases frequently occur of what is called "double consciousness," in which there is a dual personality, each one of which has a separate period to itself. Many of these are related in well-known books, but the following is taken from a recent number of the "Revue Scientifique" (May 20).

It is recorded by M. Azam, from whose dreadfully prolix narrative we glean the following facts:—The subject of the disorder is Felida X., born of healthy parents at Bordeaux in 1843. Her father, a merchant captain, died while she was young, leaving a family for the mother to support. Thus Felida's early years were spent in poverty, but she developed in the usual way till about thirteen, when, just after puberty, she exhibited hysterical symptoms, accompanied with pulmonary hæmorrhage, without apparent cause from the condition of the respiratory organs. About fourteen she suffered sharp pains in both temples and fell suddenly into an unconscious state like sleep, which lasted about ten minutes, after which she opened her eyes and entered into what M. Azam calls her second state. This lasted an hour or two, after which the fainting and sleep reappeared, and she returned to what the doctor calls her ordinary state. More attacks came on every five or six days, or less often; and her relations, noticing a change in her condition during the second life, and her forgetfulness of it on awakening, thought her idiotic. The hysteria got worse, and she suffered convulsions, which led to M. Azam, who was connected with a lunatic asylum, being called in. This was in 1858, and he found Felida a plump brunette of moderate height, very intelligent

for her position in life, but melancholy, morose and taciturn, with a strong will, and very industrious at needlework. She suffered from frequent hæmatopsies, complained of pains in the head, and hysterical symptoms. She was very anxious about her condition. Her acts and conversations showed no intellectual defect, but her affective sentiments were feebly developed. Nearly every day, without any perceptible cause, she suffered what she called her "crisis," and entered into the second state. All of a sudden, with her work on her knees, a violent pain shot through her temples, her head dropped upon her breast, her arms fell by her side, and she passed into a sort of sleep from which neither noises, pinches, nor pricks could awaken her. This lasted two or three minutes, but had formerly been longer. She woke up in quite another state, smiling gaily, speaking briskly, and trilling (*frédonnant*) over her work, which she recommenced at the point she left it. She would get up, walk actively, and scarcely complained of any of the pains she had suffered so severely a few minutes before. She busied herself about the house, paid calls, and behaved like a healthy young girl of her age. In this state she remembered perfectly all that had happened in her two conditions. In this second life, as in the other, her moral and intellectual faculties, though different, were incontestably sound. After a time, which in 1858 lasted three or four hours, the gaiety disappeared, the torpor suddenly ensued, and in two or three minutes she opened her eyes and re-entered her ordinary life, resuming any work she was engaged in just where she left off. In this state she bemoaned her condition, and was quite unconscious of what had passed in the previous state. If asked to continue a ballad she had been singing, she knew nothing about it, and if she had received a visitor she believed she had seen no one. The forgetfulness extended to everything which happened during her second state, and not to any ideas or information acquired before her illness. In 1858 her hysterical condition was well characterised, and in what M. Azam calls her "normal state" she could not taste nauseous pills; her sense of smell was deadened, and many points on her body were without sensation. The least emotion brought on convulsions without complete loss of consciousness. At this period occurred what M. Azam calls an "epiphenomenon" of the attack, which he saw only two or three times, and which a young man she married only saw thirty times in sixteen years. Instead of waking as usual from her second state, she did it in a fit of terror, and recognised no one but her husband. This did not last long, and it was only on such occasions that anything like hallucination was observed. Her two mental conditions were strikingly exhibited in reference to an incident of courtship preceding her marriage to a young

man she had known from childhood. One day, when more than usually sad, she told the doctor she was getting worse, that she was enlarging, and suffered from nausea each morning, with other symptoms of conception. After the next attack, which came soon, she said: "I remember perfectly what I have just told you, and I now confess I am with child." She had got into this condition in her second state, and was quite ignorant of it in her normal state; but this was rudely broken through by a neighbour, who thought she was shamming innocence, and told her what she had confessed in the other state. The shock to her feelings brought on violent hysterical convulsions, which required medical attention for two or three hours. Her first child was born when she was seventeen and a half, and for the two following years she enjoyed good health. When about nineteen and a half the attacks recurred with moderate intensity, and a year later she had a difficult confinement, blood spitting, and a lethargy which lasted three or four hours. From this time till she was twenty-four the attacks became more frequent, and their duration, which at first equalled the normal periods, began to exceed them. The pulmonary hæmorrhages increased, and she suffered partial paralyses, lethargies, extasies, &c. For three years, from twenty-four to twenty-seven, she was entirely in the normal state, and then for another six years, up to 1875, the disorder reappeared. Up to this date she had gone through eleven confinements, the result being two children still living. The eldest, born during one of the attacks, is nervous like his mother, and suffers from fits of terror. He is intelligent and a good musician, now sixteen years old. M. Azam gives numerous instances of the two states, and adds diagrammatic representations of them, one of which traces their changes of duration. From fourteen and a half to seventeen and a half years of age the periods of *accès*, or attack, were far between and much shorter than the normal states. From seventeen and a half to twenty and a half the normal state lasted; from twenty and a half to twenty-four three attacks; from twenty-four to twenty-seven a long normal state; from twenty-seven to thirty-two increasing *accès* states, and at the present date these states tend to become constant. M. Azam observes that "the growing diminution in number and length of the normal states leads to the opinion that they may at last cease altogether, and then her whole life would be passed in the second state." She would then have a complete personality; intelligence and memory would be there, but it would not be the old personality; she would be another person:—

"Her existence, from an external view, would include three successive personalities; the first normal, with which she came into the world; the second, divided in two by loss of memory (*amnésie*); the third new, and wholly different. Good would thus come from excess of evil, for it would

end in a sort of cure. I dare expect no other, and if it come it will be in twelve to fifteen years, at a critical age."

It is a pity that this story is not told with more completeness; for, although nothing of much importance is omitted in the preceding condensation of the original account, it leaves us without many facts which should have been recorded. After the first hysterical attack, the girl appears to have had two personalities strongly marked by differences of character. In one state she was morose and melancholy; in the other lively and agreeable. Was this state like her original condition before puberty, allowing for the usual development between one age and another? Dr. Azam gives us no information on this point. He tells us that "in her second life her moral and intellectual faculties, though different, were incontestably sound." But it was in this state she became too familiar with the young man whom she subsequently married—a transaction of which she had no cognizance in what the doctor terms her "normal" state, misusing that word, which he subsequently employs with more propriety to designate that condition in which she came into the world, and in which, while it lasted, she behaved like other children.

The second state, the birth one being her first, was that of an intermittent personality; the third of two personalities, composed of the second, and a new one, occurring in alternations, and linked together by those actions of memory and consciousness described at the beginning of this paper in the words of Dr. Carpenter.

Should a cure ultimately ensue, as Dr. Azam expects, her final state would apparently be a reversion to her birth state, with such modifications as naturally belong to lapse of time. Suppose, instead of an act of impropriety during the second state, she had committed a robbery or an assassination, no moral responsibility could have been assumed to rest upon her with any certainty by any one acquainted with her history; though without a knowledge of the previous facts no excuse would have appeared.

In these double consciousness cases the original personality is only lost at intervals; but when, in confirmed insanity, a patient supposes himself quite another character, and assumes the thoughts and feelings considered proper to such a character, the original personality, as an essential property, disappears altogether. Temporary disappearances of the real personality and its replacement by an assumed one, not only occurs in cases of insanity, but can be induced by the action on the nervous system of what has been termed "electro-biology." Thus Dr. Carpenter records having seen a lady "metamorphosed into the worthy clergyman on whose ministry she attended, and with whom she was personally intimate." He says "he shall

never forget the intensity of the lackadaisical tone in which she replied to the matrimonial counsels of the physician to whom he (she) had been led to give a long detail of his (her) hypochondriacal symptoms: 'A wife for a dying man, doctor!' No *intentional* simulation could have approached the exactness of the imitation, alike in tone, manners, and language, which spontaneously proceeded from the idea with which the fair 'subject' was possessed, that she herself experienced all the discomforts whose detail she had doubtless frequently heard from the real sufferer." *

While this state lasted the lady's own personality vanished, and she was, so far as her own knowledge and consciousness were concerned, the hypochondriacal clergyman whose ways she imitated. In a case of intoxication the loss of the real personality was only partial—a man knew perfectly well who he was, but referred all his own drunken symptoms to his family, and insisted upon undressing them and putting them to bed, affirming that they had taken too much to do those things for themselves!

The alternating form of dual personality may possibly explain some of the stories of "spiritual manifestations." A believer in these performances cannot be induced to accept as a solution of the apparent wonders the explanation that the medium is cheating him. The said medium, male or female. as the case may be, is well known, he says, to be a most veracious person, quite incapable of deceit—a most simple-minded person, unable to concoct a fraud. This may be so in the normal condition, but what has the so-called mediumistic state done? It usually comes on, more or less, like the somnolence of the girl Felida, and perhaps it transforms a reliable person into either an unwilling dupe of morbid sensations, or a cunning cheat.

Cerebral physiology is not yet advanced enough to say how the brain acts, or whether functions are rigidly localised. Professor Golz has recently shown that functions which seem quite destroyed by excision of large portions of one hemisphere, reappear after a time, if the animal subjected to the process can be kept alive. A physiologist who contributes to the "Academy," referring to these experiments as narrated in "Pflüger's Archives," says: "A belief in the existence of localized centres in the cortical substance is incompatible with the fact that lesion of any part whatever of a hemisphere is followed by one and the same train of symptoms, and with the observed restoration of the particular functions over which those centres are supposed to preside." May one who has no pretensions to be a physiologist suggest that the grey matter of the brain may have its

* "Mental Physiology," p. 523.

molecules arranged in patterns somewhat analogous to those of steel-filings under the influences of a magnet, but that in some-way the direction of the forces—or vibrations—may be changed in them. The pattern will then be different, and the position of supposed organs altered. If this be true, the search should not be for organs, but for centres of action, which in healthy brains may have fixed positions.

Leaving this and other guesses for what they are worth, we must admit that personality as we know it is a result of organisation, and that a molecular change, or a variation in the rate, or character, of those chemical actions and decompositions that are the invariable physical antecedents of thought, sensation, or volition, can instantly convert one personality into another, in which that sense of continuity which links the personality of yesterday with that of to-day may be wholly or partially destroyed. Thus we may see realised something like Circe's magical transformation of men into beasts, or a new man created surpassing the old.

Many will be startled at the notion of their personality being intermittent, not continuous; but if it consists in a series of impressions converted into consciousness, with the memory's links tying them together, the sense of the continuity of our existence, which we all feel, must be like the sensation of a continuous sound produced by successive beats at small intervals, or of continuous light from rapidly recurring impressions. If our modes of consciousness enabled us to take note of infinitesimally minute time intervals, and of infinitesimally minute molecular changes, each second would be crowded with a corresponding infinity of impressions, and a personality lasting a few minutes would be more amply filled with incidents and recollections than a human life of the longest term of years. If, with our present limitations of power to perceive minute time intervals, we were able to notice the molecular changes that took place between one pulsation of personal consciousness and another, we should suppose that nature, contrary to the old adage, did act by jumps. If a being could exist with enormously slower pulsations of conscious existence, these jumps would seem enormous, and, in an extreme case of such a supposition, the world that existed in one period might be extinguished and rearranged before another came on. The whole philosophy of such a being would differ from ours.

But if intermittence and change be the condition of our lives, is there nothing permanent to which we can cling? Physical Science has to do, in its present stage, only with facts belonging to the regions of incessant change; but man must, from his constitution, form an ideal of the continuous and the enduring towards which he aspires, and in which, in spite of doubts, he in the long run believes.

THE VIVISECTION CLAMOUR.

By the EDITOR.

HAS the outcry ceased now that a Bill has been passed by the House of Commons? We fancy not. The Bill has not satisfied those who are opposed to vivisection, while of course it has not pleased the medical profession. But the voice of the opposition has not been as loud as in the beginning of the year. Then it was shockingly violent and unreasonable. There has been, as the old saying is, "much cry and little wool," and a number of well-intentioned and equally uninformed old women have raised a banner with "cruelty to animals" inscribed upon it. This, of course, has been followed by an ignorant and withal a very noisy crowd, who have raised a special outcry against the medical profession, of abhorrent cruelty to animals. The medical profession at first took little notice of this babbling crowd; and we fear that if it had not been for the decided movement of Mr. Ernest Hart it would have been absolutely silent on the matter, and as a consequence we should have had the Act passed in its original form, which has, now that its intentions are familiar, become so objectionable to the entire profession. Mr. Hart saw the objectionable character of the proposed law, and he called public attention to the subject through his paper, the "British Medical Journal," and by a well-organised plan he got the profession together, and brought them in a body to Mr. Cross to protest against this measure (as it was then drawn up) becoming law. The occurrences at that meeting are familiar to the public. Mr. Simon and Sir W. Jenner pointed out, in terms of the most clearly logical force, the utterly unfair nature of the Bill, and the slur which it would cast upon what is unquestionably the most charitable profession in existence. While at the same time, as Mr. Simon indicated at some length, the profession of medicine is charged with cruelty—cruelty whose committal is under chloroform—the fox hunter, the harrier keeper, the deer stalker, the pigeon slayer, the rabbit snarer, to mention but

a few,* are allowed to perpetrate the utmost cruelties imaginable without aim or end other than the unquestionable torture of the animals pursued. A calm observer of the arguments on the two sides will unavoidably say, "If you desire to put down cruelty to animals, put it down first where it is both harsh and unnecessary; and then if you will, come down on vivisection,"† which is an extremely useful as well as excessively limited operation—profession, indeed, we had almost termed it.

And this view of the matter brings us to the consideration of some remarks on this subject made by the "Daily News" of August 10 or 11. This paper, in a leading article on the discussion in Parliament upon the second reading of the Bill, holds the same opinion, but it singularly enough commences at the end instead of at the beginning. It says that of course Parliament cannot undertake to legislate for the whole matter at once, but that it does well to begin with the physiologists. Why? Simply that the "Daily News" has got a craze upon the subject, and takes this unreasonable view. Surely if vivisection is to be put down at all, *i.e.* if all modes of torturing creatures are to be put an end to—an idea which is as absurd as it is impossible—that which is most general and least useful should in the first instance be considered. But, in the opinion of the "Daily News," the reverse view is correct. That which is most beneficial and infinitesimally small in its amount must be the first that is put an end to.

Apart, however, from this view of the question, there is a series of other points to be considered with which we fear the anti-vivisectionist has not at all acquainted himself. And firstly comes the fact that every animal must die at one time or another. It may be a slow death by starvation, it may be of the most intense torture—as witness a cat with a mouse or rat which she has half killed and then plays with for hours; it may be by poison—which is essential in many cases; or, lastly, it may be by some sudden catastrophe—as a flood, or a fire, or frost which slays by thousands. And to this the anti-vivisectionist will reply: But why not allow animals to live as long as they can? To this important question two answers are to be given. 1. A lower animal differs entirely from man, inasmuch as it never knows that it is going to die. If the condition of man's life were the same, death could have no earthly horror, *i.e.* death alone. If,

* Not to mention the habit of puncturing animals with iron goads for the purpose of marking them with the owners' initials, the crimping of live salmon and cod, the amputation of sheep's tails, &c. &c.

† The word should never have been used, and yet it seems difficult to substitute a better one. But the idea that it conveys of animals being flayed alive is clearly as objectionable as it is wrong.

then, we kill an animal suddenly, without allowing it to experience the sense of pain, how have we been guilty of cruelty? It has no knowledge of the intention we have had of killing it; it has the most perfect enjoyment of life to the last moment. It knows not of our having anything to do with its death. In fact, it is killed without having had an idea of mortality. Then what is the injury inflicted? 2. But there is another and vastly more important argument. This is the one which is furnished in the most convincing form in Mr. Darwin's "Origin of Species." It is in four words, "The struggle for life." I suppose my anti-vivisectionist friends are unaware of the fact, but nevertheless fact it is, that if all the animals that were brought into the world during the last fifty years were allowed to live for their natural term of years or months, as the case may be, the country would be over-run, there would not be standing room in all probability for one of us. Living would be utterly out of the question, if such a condition existed, for the lower animals alone.

Let us take a few well-known examples from Mr. Darwin's book in proof of this statement. In the sixth edition (the last but one), p. 50, of his "Origin of Species," the author gives the following example of the geometrical ratio of increase, which shows us clearly enough that for one animal of any kind that lives, thousands perish before reaching maturity; and that were it not so, the world would become uninhabitable: —

"A struggle for existence inevitably follows from the high rate at which all organic beings tend to increase. Every being which during its natural lifetime produces several eggs or seeds must suffer destruction during some period of its life, and during some season or occasional year. *Otherwise, on the principle of geometrical increase, its numbers would quickly become so inordinately great that no country could support the product. Hence, as more individuals are produced than can possibly survive, there must in every case be a struggle for existence.* . . . . It is the doctrine of Malthus applied with manifold force to the whole animal and vegetable kingdoms. . . . . Although some species may be now increasing more or less rapidly in numbers, all cannot do so, for the world would not hold them."

"There is no exception to the rule that *every organic being naturally increases at so high a rate that if not destroyed the earth would soon be covered with the progeny of a single pair.* Even slow-breeding man has doubled in twenty-five years; and at this rate in less than a thousand years there would literally *not be standing-room for his progeny.* . . . The elephant is reckoned the slowest breeder of all known animals, and I have taken some pains to estimate its probable minimum rate of

natural increase. It will be safest to assume that it begins breeding when thirty years old, and goes on breeding till ninety years old, bringing forth six young in the interval, and surviving till 100 years old. If this be so, after a period of from 740 to 750 years, there would be nearly *nineteen million elephants alive descended from the first pair.*"

Now, the above remarks—and they are but a tithe of those that could be quoted, and which we trust the reader will look out for himself—clearly prove that the amount of cruel slaughter which takes place wholesale in Nature, and which is really for the benefit of us and the other few who survive in the struggle for existence, is of so vast a character that anything in the nature of vivisection sinks beside it into such utter insignificance as to be totally unworthy of observation by the philosopher.

But if we leave these grand aspects of the question, and for a moment turn the argument against the anti-vivisectionists, what shall we see? Why, that they who cry out loudest are themselves guilty of the most intense cruelty. Which of them for instance, will avoid eating a chop, and yet what is that but the product of cruelty to animals. The poor little beast first has its tail cut off when it is a young animal. Then its testicles—if it is a male—are cruelly dissected out, *without* the influence of chloroform; and then when it has reached a fair degree of size and fatness it is killed. And if we inquire whether this act is performed under chloroform, the mere question excites a smile. Again, if it is a calf that is killed, how is the act performed? Why, most cruelly. Veal is a sort of meat which the public like to have well-bled; it is a so-called white meat. Well, the unhappy little calf is torturingly allowed to bleed to death. And how many millions of these creatures have to suffer in this manner every year! Again, will the anti-vivisectionist not destroy without hesitation all bugs, fleas, lice? Will he not, without the faintest scruple, remove and slay with a ruthless hand thousands, nay millions, of aphides; and will he not squash beneath his feet the slug and snail that inhabit his garden? Yet surely he will not tell us that creatures like *Aphis*, *Limax* and *Helix* are devoid of sensation—the latter, indeed, endowed with the most complete nervous system. Or will he not hunt the fox, or snare the rabbit, or eat his chicken that has been cruelly bled to death by the poultry-man, who places its body between his knees and slashes a huge knife across its neck, and waits, without the slightest scruple of conscience, till the fowl has bled to death? Or is he ignorant of those choice morsels of delicacy known as the *pâté de fois gras?* Does he know what torture these poor birds are put to through their whole lives in order that the

taste of the *bon vivant* may be sufficiently pleased? Or does he eat his lobster or crab, and does he know how they are prepared? Is he ignorant of the fact that they are kept out of water for hours, which has somewhat the same effect as though he were being submerged for half-a-minute say every five minutes in the twenty-four hours. But that is not all. A cauldron of boiling, steaming water is ready, and into this the still living crab or lobster, shrimp or prawn, is at once plunged. Just conceive for a moment the agony the poor brute must suffer!

Better still, let him reflect on his oyster-eating habits. A poor oyster is at least a couple of days out of his native element. Still he has sufficient liquid floating about his gills—and how beautiful these gills are the microscopist alone knows—to keep him alive till the moment that anti-vivisectionist begins his dinner or supper. Then is a knife passed through his gills and across his muscles that close the valves; next for a few seconds he is tortured by having some burning compound poured—as oil upon his wounds—upon him, and, finally, in a moment, while still in a dying state, is he plunged into the mouth and stomach of the man or woman who grieves so loudly over vivisection.

Is it not wonderful in this, which is especially the Age of Reason, to see so much of absolutely arrant nonsense talked by people whom we should take in other respects for sensible persons. Christ's argument is the most potent one in this case, and well may the vivisectionist say, "Let him that is without sin among you cast the first stone." Assuredly if they are anywhere, the Pharisees are among our opponents.

If we wanted further evidence of cruelty of the most painful kind, but which is yet absolutely unavoidable, we have only to go to any of our sea-side fishing villages. There we can see whole boat-loads of crabs slowly dying, which are torn limb from limb in the still existing condition to form bait for the fishermen. Again, look at the millions—nay, the billions—of herrings, pilchards, not to mention other salt-water fish that are caught on our own coasts year after year. Are they chloroformed or even killed at once? Assuredly not; they are allowed slowly to die of suffocation, not sudden, as in the case of a man who is drowning, but gradual suffocation, which takes some hours to complete. Then let us take fresh-water fish. Is not the salmon, if he is caught on the line, sometimes put to torture for half-an-hour or more, and is he not then most cruelly dealt with—a large, bent spear, the gaff it is called, is driven through his body and thus he is brought to land? Again, the trout, or perch, or roach, &c., is he not taken with a hook which pierces his mouth, and is he not then allowed to die the death of suffoca-

tion and pain in the basket of the angler? Take the practice which exists in some rivers of fluking. A man lying forward in the bow of a boat floating over a sandy bottom, sees the fluke—the young plaice or sole—buried in the sand, then he thrusts his spear through the animal's back, taking care that it penetrates its body completely. Next he lifts the weapon, disengages the fluke by withdrawing the spear from its body, and allowing the unhappy victim to flounder to the bottom of the boat in most excessive agony, permits it to die, which it generally does in about four hours; while he goes on spearing in most cases till he has five or six dozen of these flat-fish captured. Then, most of them still alive, they are strung on a switch of willow, which is passed through their gills and out of their mouth, and so are conveyed for miles to market.

If the above is not cruelty, we know not what is. But it might be multiplied almost infinitesimally, if we were to obtain a return of the actual amount perpetrated throughout the country.

And if now we compare with these several modes of cruelty, which are performed beneath the eyes of all, the methods of vivisection as they are practised in this country, what shall we see? We first of all find that there are in the whole land not more than from twenty to thirty persons who have to do with physiological experiments which demand operations on animals. And again, we may suppose that they work, say ten months a year, and we will further suppose that on an average they have an operation to be performed on a mammalian animal about three times a week. We are now drawing a fair estimate. Well, and what is the cruelty? The animal is tied up and in the first instance is placed under the influence of chloroform. Then possibly—it being incapable of feeling the slightest pain—a knife is inserted in its abdomen and a bit of its intestine is tied, or its skull is partly removed to see the condition of its brain or to try experiments with galvanism; or its heart or lungs are exposed for the purposes of conducting some experiment on circulation or respiration; or it may be the absorption of fat by the lacteals that the experimenter wishes to determine. Well, the animal is kept under chloroform, save in some very special cases, during the operation. Then if the injury is very slight it is allowed to recover, if the destruction of tissues has been great it is, while still under chloroform, put an end to by the simple process of "pithing."

And such cases as I have given an instance of, are, as we have seen, but few indeed when performed on the higher animals, as dogs and cats. Guinea-pigs, frogs, and newts constitute the mass of the experimenter's subjects, and we can say that there is never an example of the intense cruelty that many of the anti-

vivisectionists have spoken of. Indeed, students are exceptionally considerate to the subjects under examination, as we doubt not those who have had to perform experiments on animals will bear us out in testifying.

But if we have not a sufficient argument in favour of vivisection from the tendency to spare the feelings of animals by the use of chloroform on the part of the vivisectors, and from the fact that vivisection is as to cruelties practised every day through stupidity and sport (!!!) in the ratio of about 1 to 1,000,000, we have a last argument of greater import than any that have gone before. It is that vivisection is of use, and that it is by means of vivisection that nearly all those results have been attained which have led to establish Medicine on a partly rational basis, and which have led to advances in the art of medicine—in its widest sense—that have immensely tended to the relief of human suffering. It would, of course, be out of our power in such an article as this to give a list of the series of experiments on animals that have led to the correct ideas on the subject of digestion, circulation, respiration, absorption, secretion, and more recently on muscular action and brain-power. But those who are interested in this important question will do well to refer to a valuable article in the "British Medical Journal" of the present year, in which they will find the whole of this question amply, and fairly, and dispassionately discussed.

Is it not, then, unfair as well as unwise to present a Bill on this subject which deals exclusively with the medical profession. We think it is; yet it has been done. Mr. Cross has succeeded in getting a Bill passed through the House almost at the last moment, just as the members might be said to have been looking out their guns and cartridges, and preparing for the terrible slaughter of the twelfth. Just at this moment they have passed a Bill to hamper and impede the labours of the practical physiologist. What a magnificent subject for a clever satire. And yet, though the Bill is objectionable to the medical profession in some of its features, more especially so in its prevention of vivisection in any medical school, and in its application to vertebrate instead of warm-blooded amimals, the anti-vivisectionists are outrageous with Mr. Cross for his timidity (!!) in leaning towards the medical profession. Thus has the Home Secretary fallen into the position usually occupied by the person who sits between two stools.

We think that the only member of the House of Commons who spoke rationally and calmly on the subject was Mr. Robert Lowe, and we thoroughly sympathise with all the observations he made upon the subject.

The anti-vivisectionists are dissatisfied, and mean to open the battle again next session. But things will be changed then,

and we doubt not that much of the wild frenzy so often associated with the petticoat will have had time to cool down, so that altogether while we do not approve of the form legislation has taken, we are yet hopeful as to the future, and we would urge upon our medical readers to use all their influence with their parliamentary friends towards the utter abolition of this Bill in the next non-Disraelian session of Parliament.

REVIEWS.

GEOGRAPHICAL DISTRIBUTION OF ANIMALS.*

TO review a work like the present one would be, except to some half-dozen writers in the same field, an absurdity, if not an impertinence. To give even an analysis of its contents would require infinitely more space than that of our entire "Reviews." What can we do, then, in the present instance? We can just briefly give an account of the nature of the volume, of what it is the author has attempted to do, and of the time and space which his labours have occupied, and their results have been embodied in. We may say that to us Mr. Wallace appears to have very faithfully succeeded in doing what he wished, viz. that his "book should bear a similar relation to the eleventh and twelfth chapters of the 'Origin of Species' as Mr. Darwin's 'Animals and Plants under Domestication' does to the first chapter of that work." In this respect we may say that the analogy is complete. Nay, more; we fancy that Mr. Wallace's labours, as put forward in the two splendid volumes now before us, will be read when Mr. Darwin's volumes on "Animals and Plants" will have fallen into desuetude. Whether it will have as long a lease of existence as the "Origin of Species," it is, of course, impossible to say; but of its great importance as a scientific treatise there cannot be the least doubt in the mind of anyone who is acquainted with the history of Nature; and this all the more so because it is a work *sui generis*. Mr. Wallace has had to undertake an entirely novel labour in preparing those volumes, and the result is the production of a work that is completely new. Not only so, but it is executed by one who is a thorough master of the subject; by the man who might, but for his extreme modesty, be now in the proud position which Mr. Darwin holds—that of the first naturalist in the world. It is clear, therefore, that we may anticipate a subject dealt with in a philosophic spirit, as such a subject especially requires. And ere we take a glance at Mr. Wallace's labours, let us consider how long they have occupied. This is shown in the following passage from the preface to the work. Mr. Wallace says:—"The detailed study of several groups of the birds and insects collected by myself in the East brought prominently before me some of the curious problems of geographical distribution; but I

* "The Geographical Distribution of Animals; with a Study of the Relations of Living and Extinct Faunas, as elucidating the Past Changes of the Earth's Surface." By Alfred William Wallace. With Maps and Illustrations. 2 vols. London: Macmillan & Co. 1876.

should hardly have ventured to treat the whole subject had it not been for the kind encouragement of Mr. Darwin and Professor Newton, who, about six years ago, both suggested that I should undertake the task. I accordingly set to work, but soon became discouraged by the great dearth of materials in many groups, the absence of general systematic works, and the excessive confusion that pervaded the classification. Neither was it easy to decide on any satisfactory method of treating the subject. During the next two years, however, several important catalogues and systematic treatises appeared, which induced me to resume my work, and during the last three years it has occupied a large portion of my time."

And what, the general reader will probably ask, has the author of the work attempted? We shall endeavour to explain. Animal life exists in almost every portion of the globe; indeed, for convenience, we may assume that on every portion of the earth animal life is apparent. But we find as we travel through, suppose the forests of Brazil, or the regions of Upper India, or in the Arctic provinces, or again in Australia, a very different set of animals. Thus in one we find all the creatures have, more or less, marsupial pouches, as the kangaroo; in another we find the old-world apes; in a third we find elephants, and so on. Then again in point of time—that is, suppose a million of years—we find similarly a peculiarity of distribution of animal existence. For example, we find one type of animals succeeding another as we pass from the older to the more recent fossiliferous deposits. Now in both these cases it is of importance to find out how it happens that such different forms of life came in these localities, both of space or of surface, and of depth. Of course, if you took the view that every animal was separately created, there would be at once an end to the whole discussion; for then you would have taken it for granted, not that the animal of any particular locality arrived there ages ago from elsewhere, but that it was created on the spot. That would be an exceedingly singular view; and, unfortunately for his convenience, the scientific man must give it up at once. Then for him comes the question, How did these several races extend from one part of the world to another? Why, for example, should we find fossil in this country animals the same as those now living in Australia?

It is to this excessively difficult task that Mr. Wallace has partly devoted his attention, though of course he has had in the first instance to endeavour to arrange all the animals which are on the globe into a series of groups, so as to have those that are closely related to each other as to distribution as much together as possible. And Mr. Wallace has discussed the various schemes that have been suggested by different writers, and he has come to the conclusion that there are—as long since proposed by Mr. Sclater—six regions into which the world may be divided. These are (1) *Palæartic*, which includes North Europe, Mediterranean, Siberia, and Japan. (2) *Ethiopian*, which comprises East, West, and South Africa and Madagascar. (3) *Oriental*, which includes Hindostan, Ceylon, Indo-China, and Indo-Malaya. (4) *Australian*, including Austra-Malaya, Australia, Polynesia, and New Zealand. (5) *Neotropical*, including Chili, Brazil, Mexico, and Antilles; and (6) *Nearctic*, comprehending California, the Rocky Mountains, the Alleghanies, and Canada. Now taking these great groups as the primary division of the animal world, the author traces every genus

of vertebrate animals along these lines—mammals, birds, reptiles, and fish. And with this labour is the greater part of these two huge volumes filled. The general reader will find a series of engravings arranged as plates throughout the volume, which will much help toward putting the ideas of the author clearly before him. Each of these represent a division, and shows together the animals that are included in it. Mr. Wallace thinks them well executed; but we do not at all agree with him, for we consider them hard, and generally badly drawn. Still, they lend a great interest to the work. The maps, which are abundant, are also very valuable; but we think the author's method of indicating the different degrees of height by differences of shading is extremely perplexing, for the distinctions are not absolute but gradual, and the confusion resulting from this is very great.

But even admitting these slight objections, the treatise is one of the highest scientific importance; and for ourselves we must express our extreme gratitude to the author for his labours—labours executed purely for the benefit of science, and which can never be repaid by any monetary return they may bring.

WHAT IS CHOLERA?*

ON this important subject we have now before us two works, which, though differing in character—the one being an elaborate Report of the progress of a single epidemic in America, which extends over more than 1,000 pages; the other an equally important treatise on Asiatic cholera as it presents itself to our notice in India—have yet much that is in common between them. We shall take the American volume first. In this most valuable Report on the progress of a single epidemic, it is proved conclusively that the disease did not arise spontaneously, but was introduced by immigrants from European countries. Of course there is a certain amount of evidence in this work that tends to an opposite conclusion, but it is almost exclusively negative in character; and where any opinion adverse to the belief in the spread of the disease from Europe, and primarily from India, is expressed, it is generally the idea of some individual reporter who has had little or no experience in the subject.

Throughout the entire work, which includes the various smaller reports of hundreds of physicians, we notice the same features in the history of the disease that are presented by every undoubted case of Asiatic cholera. And we are glad to observe that the author, or rather the editor of the work, points to the extreme importance of attending to the very first symptoms of diarrhœa that present themselves. It is really because of the neglect of this matter that cholera produces such an intense fatality. We observe that these American cases are precisely similar to those which we have seen in this country, and those which were unfortunately amply abundant in the East

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* "The Cholera Epidemic of 1873 in the United States." By John M. Woodworth, M.D. Washington: Government Printing Office. 1875.—"A History of Asiatic Cholera." By C. Macnamara, F.C.U., Surgeon to the Westminster Hospital. London: Macmillan & Co. 1876.

Indies. That is to say, first there is diarrhœa, which is painless but copious; then pain, vomiting, cramps of the extremities, sinking of temperature, blueness of skin, collapse and death. It is to be noticed that these Reports include those both of the civil and military services, and that the latter is infinitely the most lucid, full and scientific. There is also in this volume a valuable notice of previous epidemics, from the time of Hippocrates (B.C. 460-370) down to the year 1873. We note also an important series of maps and plans, by which the reader is greatly assisted in arriving at his conclusions. But perhaps the most valuable part of this American Report is the portion devoted to bibliography. This has been carefully edited by Mr. J. S. Billings, and extending as it does over more than 300 pages, includes, in alphabetical order, everything that has been written on the subject since it began to have any literature connected with it. This one feature will make it a treatise that must be referred to by all writers and readers on the subject of cholera. Indeed this Report is a most thoroughly creditable one.

Of Dr. Macnamara's work—which is in some respects quite a different one, having to do rather with the origin both as to locality and nature of the disease—we have not the less highly to speak in praise. It is a work which the author has addressed to the educated lay reader as well as to the physician, but we fear the number of persons outside the medical world who are interested in cholera is extremely small. However, the book is of importance to all who consider the question, How is this plague to be arrested? And we are glad to see that Mr. Macnamara holds distinctly to the theory that cholera is only communicable through the swallowing of some portion of the discharge which has come from a patient suffering with the disease. Thus he believes—as we and nearly all rational physicians do—that the disease is not by any means infectious, and that it is not contagious in the ordinary sense of the word. He believes, then, that it is conveyed from town to town, and from country to country—in point of fact, from Bombay to New York—by the medium of water. Of course ships may not have their water infected—though there are undoubted cases cited from the Crimean war, in which the water was the means of convoy—but then the disease has not had time during the voyage to die out, and thus cases are brought to the different intervening ports—let us say, between Bombay and London. And the author deals exhaustively with the many theories that have been, from time to time, put forward. To account for the propagation of the malady thus, he deals very fully with the influence of winds in spreading the disease; and he shows, in the most conclusive way, that the atmosphere has no agency whatever in the distribution of cholera. In a similar manner he treats the subject of food and meteorological influences, as heat, fog, &c., and he shows that they have no influence over either the origin or the spread of this fearful affection.

We think that Mr. Macnamara has, on the whole, gone too fully into the several courses that cholera has taken from time to time, for we are of opinion that a very brief abstract would have been amply sufficient. We note also that he has said almost nothing as to the primary source of the fungus—for fungus it undoubtedly is which originates the malady—and we think that Dr. Lewis has already done some good work

in this direction. Still the book is a most valuable one, and must be carefully read by all who are interested in the questions, How does cholera come in the first instance? and how is it passed from human being to human being in the second?

THE BOOK OF WEATHER SCIENCE.*

WE doubt whether Messrs. King could have chosen a better subject for a scientific book, and we are sure that, having made the selection, they could not have hit upon one who is not only one of our best meteorologists, but who is in addition a clear and concise writer, more successfully than they have done in the case of this volume, called "Weather Charts and Storm Warnings." Mr. Scott has here given us an insight into the working of the Meteorological Office, and the production of those maps which everyone now sees in the daily papers, but which few, we fear, understand. However, it will be discreditable now not to comprehend clearly the tracings that are every day published in the press. The author has had a double difficulty to deal with in the production of the present volume, for he has had to bear the brunt of issuing the first work that has ever appeared on the subject; and, in addition, he has had to prepare a series of statements on some points that are, as he is obliged to confess, as yet hardly satisfactorily understood. Still, he has laid the question on which he has written clearly and without prejudice before the reader, while at the same time he writes as a true scientific worker, who is prepared to see many of the theories now adopted completely annihilated in course of time. He says that he aims at discussing the present condition of "weather knowledge, as distinguished from the science of meteorology itself. . . . In treating of a science now in process of rapid development, it can only be expected that every year will add to our knowledge, and that many of the principles stated in these pages will be extended or modified by the results of subsequent experience."

The book is divided into eight chapters, which cover about 150 pages. These deal with the following questions:—The materials available for weather study; the wind; the barometer; gradients; cyclones and anti-cyclones; the motion of storms and the agencies which appear to affect it; the use of weather charts; and, lastly, storm-warnings. Then follow a series of appendices, A, B, C, and D, which refer to certain remarkable weather reports taken by automatic instruments in Valencia, Aberdeen, and Falmouth. Of course the author has not much to say on the first subject in the above list. The materials are almost as well known as the weather-glass itself. Still, some of Mr. Scott's remarks must strike the general reader as of importance. For example, in telling us that the chance of rain depends to a great extent on the degree of humidity of the air, and that "if

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* "Weather Charts and Storm Warnings." By Robert H. Scott, M.A., F.R.S. With numerous illustrations. London: Henry S. King & Co. 1876.

we are dealing with reports from an extensive tract of country, as North America, or the continent of Europe, the distribution of the moisture or of the vapour-tension will afford great assistance in tracing out the probable motion of storms." He here shows us, what many are not aware of, the great importance of the wet-bulb thermometer, for it is by means of this instrument that the amount of moisture in the atmosphere is ascertained. And further on he points out the important fact that most of our meteorological stations are situated at the sea-side, where the atmosphere is almost always laden with moisture. They are, therefore, by no means so valuable for the determination of advancing rain as the more inland observatories.

In regard to the barometer, or rather the weather-glass, taken by itself as a guide to the state of the weather, Mr. Scott points out that this method is utterly absurd. We must quote him again on this point:—"Let us take, for example, the word 'Change.' This is placed opposite the reading '29·5 inches,' which reading is naturally supposed to be taken at sea-level. If the barometer be removed to a station situated say 500 feet above that level, the corresponding reading will be about 29·0 inches, so that the whole scale will be half an inch out, and the error will be greater the more considerable the height of the station. The lettering is, therefore, again wrong, because it does not take account of the necessary reduction of the reading to sea-level. Once more, the range of the barometer is far greater in winter than in summer, so that the reading which corresponds to 'Fair' should be much nearer to 'Change' in summer than in winter . . . *The words are, in fact, little less than utter nonsense.*"

The subject of gradients is very fully gone into by Mr. Scott, and the reader will follow his remarks with considerable interest. The chapter on cyclones and anti-cyclones shows us clearly how much work remains to be done to satisfactorily clear up our knowledge on the subjects. Indeed, on this question, which is complex enough, we do not see that very many conclusions can be drawn. However, such as they are, the author gives them to us. Another problem, that is certain to be more clearly ascertained as researches go on, is that of the motion of storms and the agencies affecting it. On this, too, Mr. Scott has told us all that is known. Of much interest are the author's observations on the connection (for there is an undoubted connection) between sun-spots and cyclones. He says, "Of late years Mr. Meldrum, of the Mauritius, has shown that the cyclones, for which that district of the Indian Ocean enjoys an unenviable notoriety, have been more frequent in some years than in others, and that these epochs of maximum frequency occur at intervals of about eleven years, coinciding with those of maximum sun-spot frequency." He then asks the question, why it has been left to Mr. Meldrum to arrive at this discovery? and he gives two excellent reasons why it should have been so.

Then he points out the nature of the well-known storm-signals—which were frequent enough in Admiral Fitzroy's time—and he shows us what we certainly are surprised to learn, that there has been great success in the prophetic department of the Meteorological Office in regard to its signals to Hamburgh. He says, "A system of warnings for Hamburgh, from our office, has been in operation since 1867, and the general results are that

301 warning messages were issued from London in the course of seven years. Seventy-two per cent. of these warnings were followed by gales, while in only three cases did the storm outrun the message." Although this result is manifestly better than any that has been attained with regard to storms affecting this country, still we may hope that further experience will tend to render our own storm-signals more productive of good than they are at present; although Mr. Scott says, on p. 144, that "we are able to maintain a general success of nearly eighty per cent. for our warnings"—a circumstance which will justify those who maintained that the Meteorological Office was wrong when at first they discontinued Admiral Fitzroy's system.

And with this subject we have come to the end of "Weather Charts and Storm Warnings," which we must pronounce, in conclusion, to be one of the most interesting and instructive volumes that has reached our hands for a very long time.

THE MOON.*

THIS book was undertaken with the view of promoting the study of Selenography. Unfortunately it is so large and costly a work as to be calculated to act as a deterrent from that rather dry, because heretofore unproductive pursuit. It was very well for Mr. Nasmyth to produce a costly book on the moon, because his work was intended for all who take interest in the wonders of astronomy; and, adorned as it was with many very elaborate and beautiful photographs, could not fail to be generally attractive. But a work like the present can only meet the wants of a very limited section of the astronomical public, and probably only a small portion of those even who take special interest in selenography would care to purchase a book whose retail price is a guinea and a half. We fear, therefore, that Mr. Neison may have to wait long before the second edition referred to in the preface of this work is called for, unless indeed he has prudently restricted the first edition to a few hundreds instead of the customary thousand, or the larger numbers to which publishers extend the first editions of more attractive works.

Regarding this work as a general treatise, to which character it in some degree aspires, we must confess we cannot perceive its *raison d'être*. It gives the history of lunar research too sketchily to be of much value in that respect, and only touches upon the peculiarities of the lunar motions, without a full account of which a general treatise on the moon is like Hamlet without the Prince of Denmark. Mr. Neison enters into a well-meant but perfectly futile attempt to show that those astronomers have been altogether mistaken who have asserted that the moon "has no atmosphere of any appreciable importance," and generally that the moon is at present an utterly unfit abode for any kind of life, animal or vegetable. And here we cannot but notice a very objectionable feature of the work—the cavalier, we

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* "The Moon: and the Condition and Configurations of its Surface." By Edmund Neison, F.R.A.S. London: Longmans. 1876.

may almost say insolent tone, in which our author sets on one side the opinions of men like Sir J. Herschel, Mädler, and others, in some cases without even saying whose opinion it is which he thus rejects. One sentence of the preface is so characteristic in this respect, that we quote it in full :—"As it has been in general assumed, entirely without any foundation, that the moon can have no atmosphere of any appreciable importance, it has been considered desirable to point out how entirely baseless this view is, and to show not only that the moon may possess an atmosphere relatively little inferior to the earth's, but also that the entire evidence we possess on this subject is strongly favourable to the moon possessing such an atmosphere." We need hardly say that Mr. Neison entirely fails to carry out this vaunt, or that the great astronomers whose opinions he so cavalierly sets on one side have not adopted baseless views or made assumptions "entirely without any foundation." We believe Mr. Neison is a comparatively young man, and he has manifestly much to learn about the *convenances* of scientific writing. Will he pardon us if we point out that whereas it is perfectly legitimate and proper for any man to advocate views opposed to those held by the highest authorities, it is altogether improper to assert of the opinions of such men that they are "utterly baseless," "entirely without foundation," and so forth. Even the wisest, as we all know, may err, but only the most foolish would adopt opinions without a particle of evidence in their favour, and the great men whom Mr. Neison professes to controvert were not foolish by any means.

To indicate the character of Mr. Neison's reasoning on these matters it will only be necessary to remark that after expressing the opinion that the surface density of the moon's atmosphere may be equal to about 1-300th part of the density of our earth's atmosphere, he maintains that this exceedingly rare air would decrease considerably the heat of the lunar day and the cold of the lunar night. It is true he believes that immense quantities of aqueous vapour rise into the upper strata of the lunar atmosphere, interrupting the solar heat, and "preventing the solid body of the moon ever rising above its mean value" (whatever that may mean), while "in the same way the fall of the lunar temperature during the long lunar night would be prevented by a similar cause"—a most astounding assertion in the presence of the established laws of physics and of what is known about the moon's condition.

The selenographical portion of the work may be divided into two sections, first, the maps and descriptive matter; secondly, the formulæ. The former section is on the whole good, but would have been much improved if the maps had been more uniform in character; as it is, some details are introduced which would require a good telescope to show them, and some omitted which a very moderate telescopic power would reveal. The formulæ are for the most part useless to selenographers. Those who can follow them as they appear—of whom we may be permitted to doubt whether Mr. Neison himself is one—could have no occasion for them in this book; but those not sufficiently acquainted with mathematics to obtain all the formulæ necessary for selenographical work, would most assuredly not be benefited by this section of the work, which does not possess a single explanatory plate or woodcut, though it extends over nearly fifty pages.

THE WARFARE OF SCIENCE.*

PEOPLE who have received sufficient education to tell them of the history of Europe during the middle or dark ages, are aware of the fearful contests which took place between religion—then under the guardianship of Papal administration—and science, the pursuit then of the few. The history of Galileo, Vesalius, Copernicus—or Kopernik, as he is sometimes styled—which ought to be popularised most extensively, shows what the Church has done towards repressing knowledge in times gone by. And in this respect we must not be too severe on the Roman Church, though it was powerfully excited against the progress of science; for we find that even Luther himself, whom Protestants are always singing the praises of, was in his way as completely opposed to scientific advance as many of the Papal inquisitors. Dr. Dickson shows us this in the following passage:—"Justice compels me to say that the founders of Protestantism were no less zealous against the new scientific doctrine. Said Martin Luther, 'People gave ear to an upstart astrologer who strove to show that the earth revolves, not the heavens or firmament, the sun and the moon. Whoever wishes to appear clever must devise some new system, which of all systems is, of course, the very best. This fool wishes to reverse the entire science of astronomy. But Sacred Scripture tells us that Joshua commanded the sun to stand still and not the earth.'" And to this quotation from Luther might be added many others, more especially from the writings of Philip Melancthon, all of which condemn, not less strongly than the Papal powers, the scientific philosopher.

But it is remarkable that the power of the Church—whether that of Rome or of England—is exerted as industriously, if not as successfully, against science in the present day as it was in the times of Pope Paul V. There is no incorrectness in the following statement which is made by Dr. Tyndall in the preface to the book before us:—"In our day the Roman Church above all others aims at the revival and perpetuation of this wrong—striving after a domination which she never fitted herself to exercise, and which if exercised could only bring calamity on the human race. Ignorance alone can give her any chance of success; but with ignorance to work upon, her conduct in Spain may be taken as a gentle illustration of what it would do elsewhere. Gentle, I say, because unabashed as she seems, we must ascribe some power of restraint to the knowledge that her operations are carried on in the full blaze of intellectual day."

But if we would see how far religious opposition to freedom of thought has gone even within the past twenty-five years, we have only to refer to two instances which Dr. White very wisely calls attention to. In 1864 a number of men in England drew up a declaration which was signed by a definite few, who expressed their "sincere regret that researches into scientific thought are perverted by some in our time *into occasion for casting doubt upon the truth and authority of the Holy Scriptures.*" Nine-tenths of

* "The Warfare of Science." By A. D. White, LL.D., President of Cornell University, with Prefatory Note by Professor Tyndall. London: Henry S. King and Co. 1876.

the leading scientific men in the country refused their signatures, whilst Sir J. Herschel, Sir J. Bowring, and Sir W. Hamilton administered through the press the most crushing rebuke to those who had got up the circular. And finally Professor De Morgan, in a parody, "covered memorial and memorialists with ridicule." We trust that this little book will help to allay some of the fears which religious people feel in regard to science, which can never come into conflict with genuine religion. The one is a mass of teaching all deduced from facts. The other has to do with those peculiar yearnings which all possess at some time or other, and which cannot be suppressed by any amount of teaching in minds of a certain stamp.

ELEMENTARY BOTANY.*

THIS is, with one or two exceptions, an admirable little work, well illustrated and cleverly written, while its woodcuts are most numerous—a point of immense advantage—and are generally well done. Withal, when we state that it is published at sixpence—which means that it is sold at about fourpence—our readers will be not a little astonished. The writer, Mr. W. Bland, is master of the Educational School at Duffield, and he has certainly done well in getting up such a book for his own and other pupils. The work is well done, save in the horrible selection of that barbarism of barbarisms, an artificial system of classification. Why did not Mr. Bland choose the excellent natural system to be found in Bentham's "Handbook of the British Flora"? We think, too, the postscript by the Rev. J. Smith might have been written in plain English, or omitted altogether. We object to sentences for children such as "Corrosive sublimate *plus* saturation," and that singularly unnecessary and pedantic expression, "culinary heat."

A POPULAR FERN BOOK.†

WE think there is an abundance of books in the market on ferns; indeed, we may almost say that it is glutted. And therefore, unless a book is written on this subject which has peculiar views of its own which tend to make it distinct from those which have gone before it, we think there is no justification for its publication. Well, now, can we say aught of the work before us that will help us to place it in a category of its own? We think we can. It is a book written by one who is evidently an intense lover of fern-life, and it is intended rather to awake a love for fern culture than to help the amateur botanist to study the group as a whole, or to find out any particular species he may have come across in his wanderings in the

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* "Notes of Lessons on Elementary Botany." By W. Bland. Part I., First Year's Course, with 140 illustrations. London: Bemrose and Sons.

† "The Fern Paradise. A Plea for the Culture of Ferns." By F. G. Heath. Second edition. London: Hodder & Stoughton. 1876.

country. Its *raison d'être* is therefore distinct enough. How has the author done his work? On the whole, well. Still there are objections to be raised. In the first place we accuse him of a desire to make a book. This is quite clear. Indeed, everything that has been said in the nearly 300 pages might have been easily put into half that number. Then, again, the author is lax and talkative where he should be precise and terse. We note also an utter absence of illustrations, save the coloured frontispiece. This is a mistake, especially as the author admits that his descriptions are inexact. How can the reader use the book? Still it has passed through a second edition, and we suppose that is an adequate reply to our queries. Nevertheless we fancy if illustrations were employed it would soon reach a third edition.

VICTORIAN GEOLOGY AND PALÆONTOLOGY.*

THE catalogue by Mr. Ulrich contains a detailed description of an extensive series of rocks, illustrative of the geology of Victoria, the classification being mainly based on that given by Dr. Zirkel in his work on Petrography. Interspersed throughout are some valuable notes on the distribution and characters of the various rocks, and their modes of occurrence. From these it appears that the basalt rocks are extensively developed in Victoria, being estimated to occupy from 6,000 to 7,000 square miles of the surface. They appear to belong to two distinct periods; the older, and more widely distributed, occurred at the end of the miocene epoch, the newer eruption commencing towards the close of the pliocene epoch, and seems to have continued into the most recent post-pliocene times, the country occupied by the two basaltic flows differing very much in its physical features. Of the Palæozoic series, the Silurian rocks occupy a large area, and are estimated to be about 35,000 feet in thickness, and as regards the mineral capabilities, the Silurian, as a whole, is the most important rock formation to the gold miner, on account of its containing the matrix of the gold in the number of veins, lodes, and reefs of quartz that traverse it.

The Tertiary strata, however, whether regarded in their economical or physical aspects, occupy by far the most prominent place in Victorian geological history—either of sedimentary or volcanic origin. The strata of this period are distributed over fully one-half of the surface of the colony, varying in thickness from a few to more than 300 feet, and ranging from the sea-level to elevations of over 4,000 feet. They are of miocene, pliocene, and recent ages, the two latter having a far wider surface range than the older marine tertiary deposits, and are the most important, as they embrace the auriferous drifts which, with the associated streams of basalt-lava, belong at least to three distinct periods of deposition, the earliest not older than the pliocene, and the newest is probably due to causes still in operation.

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* "A Descriptive Catalogue of the Specimens in the Industrial and Technological Museum, illustrating the Rock System of Victoria." By G. H. F. Ulrich, M.E., F.G.S. Melbourne: 1875.—"Prodomus of the Palæontology of Victoria." By Prof. F. McCoy. Decade III. Melbourne: 1876.

The decade by Professor McCoy is a continuation of the description of the characteristic Victorian organic remains, and contains a number of illustrations of the fossils of the tertiary formations, and some other forms interesting to the geologist. Among these is much additional information as to the characters and dentition of the singular animal, the marsupial lion of Owen, and illustrations of some new tertiary species of *Cyprœa*, *Aturia*, *Pleurotomaria*, and *Trigonia*, the latter two genera abounding in the mesozoic rocks, but of excessive rarity in recent and tertiary times, so that the species here described form an interesting addition to the history of the distribution of these genera in time and space. Some species of trilobites are also figured which are absolutely identical with forms abounding in the upper Silurian rocks of Europe—one of them British, and the other common in the Silurian basin of Bohemia—showing the wide range of similar species of trilobites, like those of graptolites described by Prof. McCoy in a previous decade in the seas of the palæozoic period.

THE SHELL MOUNDS OF FLORIDA.*

THIS memoir by the late Professor J. Wyman relates almost exclusively to the shell mounds and shell fields on the banks of the St. John's River in East Florida, which, as far as at present known, were the dwelling-places of the earliest inhabitants of the region through which this river flows. But little is known of the origin of these antiquities, which were long considered to be of natural and not of artificial origin. These mounds consist entirely of certain species of *Unio*, *Ampullaria*, and *Paludina*, of which the latter forms the largest portion of every mound, and with a few Unios the whole of some. Sometimes one species forms considerable deposits by themselves without the admixture of the others, as if at certain times they had been exclusively used as food; occasionally other shells, as Melaniæ and Helices are found, but in very small numbers. The mounds of St. John's appear to differ from the shell mounds of other rivers of the United States, which latter consist almost exclusively of Unios, those of St. John's being peculiar as affording the only, or at least the chief, instances in which the Ampullarias and Paludinas have become to so large an extent articles of food. They are also different as to their characteristics from the mounds on the sea-coast, which are composed entirely of marine species of shells.

These mounds are almost in all cases built close along the banks of the river, usually in the form of long ridges parallel to the shore, rising sometimes to the height of twenty feet or more, and resting either on ridges of sand or river mud, or on land slightly raised. They are often placed at the union of the river with a lagoon or creek, or at the outlet of a lake, such places probably giving the natives ready access in canoes to large areas for

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* "Freshwater Shell Mounds of the St. John's River, Florida." By. J. Wyman, Peabody Academy of Science. Fourth Memoir. Salem, Mass. 1875.

hunting and fishing, as well as for bringing together the different species of shells of which the mounds are made up.

From the various interesting facts brought together in the paper, and especially from the presence of fire-places, ashes, calcined shells, charcoal, and implements, together with the bones of edible animals, and occasionally those of man, found at various depths from top to bottom, and the absence of everything which might have been made by the white man, it seems certain that these mounds were the accumulations by, and the dwelling-places of, the earliest Indian inhabitants during the successive stages of their formation; and that some of them, and perhaps all, were completed and had been abandoned before the white man landed on the shores of Florida, for the signs of their great age are to be found not only in the mounds being covered by dense forests, but in their partial destruction by the river, the growth of swamps, and the consolidation of the shells through the percolation of water charged with lime.

AMERICAN GEOLOGICAL SURVEYS.*

OF the eight papers contained in the third number of the "Bulletin," two are devoted to geology, three comprise entomological subjects, one on a contour map of the United States, and the last on the grammatical structure of the Nez Percés language. Dr. Hayden's paper, which occupies the largest portion, and full of interesting details, is simply intended to render the beautiful pictorial sections which accompany it more intelligible to the general reader. They represent the scenery along the valleys of the Lower Gallatin and Madison rivers, together with two fine illustrations of different portions of the Yellowstone valley. The geological structure of the district is described, and some important facts are stated respecting the channels of the rivers of the West, from which it appears they do not necessarily lie along any fissures, anticlinal and synclinal depressions, but seem to have, in the majority of cases, cut their way directly across the line of fracture, thus carving out deep gorges through the loftiest mountain ranges. The notes on the tertiary and cretaceous periods of Kansas, by Mr. Mudge, shows that the former rocks occupy about 9,000 square miles, and that the cretaceous cover more than half of the surface of the State. In the paper by Mr. St. John, a further account is given to that previously published by Dr. Hayden, of the region of the headwaters of the Canadian river, which for its geologic interest and scenic features is considered not to be excelled by any similar extent of country in the West; from which it appears that the upper basin of the Canadian is underlaid by cretaceous strata, overlying which are deposits of tertiary age, including portions of the

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* "Notes of some Geological Sections of the Country about the Headwaters of the Missouri and Yellowstone Rivers." By F. V. Hayden, &c.—"Notes on the Geology of North-Eastern New Mexico." By O. St. John.—"Bulletin of the Geological and Geographical Survey of the Territories." Vol. II., Nos. 3, 4. Washington: 1876.

great lignitic formation, with its deposits of coal and iron. There are also evidences of considerable igneous action of recent date, as dykes are found traversing both the cretaceous and tertiary strata, altering the rocks through which they pass. Among the peculiarities of this basin of the Canadian is the terrace-like steps or benches—variable in height and distinctness of definition, according to the nature of the deposits—and which are the records of early shore-lines in the drainage of the ancient waters which occupied the region.

Other portions of the district are also described, of which many interesting details of the geological and physical structure are given. The remaining papers of No. 3, which complete the second volume of the "Bulletin," comprise descriptions of some carboniferous and cretaceous fossils from Vancouver's and Sucia Islands and other North-western localities; a note on a singular cretaceous crinoid, apparently related to Marsupites, from the Neobrara group of the Upper Missouri; and remarks on the geographical variation, especially as to size, among North American mammals.

SCIENTIFIC SUMMARY.

ASTRONOMY.

A HITHERTO Unnoticed Inequality in the Moon's Motion in Longitude. —Professor S. Newcomb, of the Washington Observatory, has noticed an irregularity of the moon's motion which at present has received no explanation. After applying all the known corrections to the observations of the moon at Greenwich and Washington, from 1862 to 1874, "I was surprised," he writes, "to find systematic errors outstanding which could not be corrected in the lunar elements. Of their reality there could be no serious doubt, because the Greenwich and Washington observations agreed in showing them. At first I was disposed to attribute them to inequalities in the surface of the moon, but a more careful observation showed that they were periodic, and developed on the moon's longitude, being at first positive when the longitude was between 180° and 360°, and negative in the first semi-circumference. But this was found to be subject to a sensible alteration, the point of maximum positive error moving forward to about 0°, and that of negative error to 180° in the course of a few years' observations." The maximum amount of this inequality is 1″·5, and its period is 27·4304 days ± 0·0040 d. "There is a large preponderance of probabilities," says Professor Newcomb, "against the real period being either less than 27·42 days, or more than 27·44 days. No known term in the moon's longitude falls between these limits. The moon's sidereal period is 27·32 days, and her anomalistic period is 27·55 days, so that the new term falls half way between these two. The non-accordance of this period with that of any term heretofore sought for is the reason why this term has not before been noticed. A term of unknown period would not be remarked unless its magnitude was such as to visibly affect the individual comparisons of theory with observations, and Hansen's Tables, as corrected, are the first ones of which the residual errors are so small that a term of 1″·5 would be remarked in the comparison with observations."

The Atmosphere of Venus. — In 1849 Mädler and Clausen published some observations of Venus when near inferior conjunction, which showed that the sun's rays underwent a considerable refraction from the atmosphere of the planet. Mädler used the following formula:—

Let V = the angular distance between the centres of the sun and Venus; x, the prolongation of the cusps of the planet; S, the semi-diameter of the

sun; ϕ, the radius vector of Venus. Then the horizontal refraction ζ_0 of the atmosphere of Venus was found from the equation,

$$\zeta_0 = \tfrac{1}{2}\left(\psi - \frac{S}{\phi}\right), \text{ where } \sin\psi = \sin V, \sin x.$$

By this formula Mädler found the horizontal refraction of the atmosphere of Venus to be 43″·7.

Strangely enough, the Astronomer Royal to whom Mädler sent his result, and who communicated an account of it to the Astronomical Society, detected no flaw in the formula, nor did the editor of the "Monthly Notices," who published it. Prof. Lyman, also, of Yale College, Conn., U.S., employed the formula unchanged to reduce his measures of the prolongation of the cusps, as observed with a 9-in. refractor, in 1866 and 1874, getting 45″·3 and 44″·5 for the horizontal refraction of the planet's atmosphere. We may learn from this how necessary it is to examine carefully every formula one may have occasion to employ in scientific work, no matter what authorities may have given it their sanction. For, after all, the formula was wrong. Mr. Neison, in a Paper recently read before the Astronomical Society, says, "A note by Mr. R. A. Proctor, in the 'Astronomical Register' (October 1875), induced me, as soon as I had the leisure, to examine this formula of Mädler's, when it was at once apparent that as it had been employed by Mädler and Lyman it was defective. Instead of the angle V, or the angular distance between the centres of the sun and Venus, as seen from the earth, which they had used, they should have employed the supplement of the angle between the sun and earth, as seen from Venus. In consequence of this error, the value which they have deduced from their observations for the horizontal refraction of the atmosphere of Venus is incorrect." Making the correction indicated by Mr. Proctor, Mr. Neison obtains as the mean result of Mädler's observations 54″·43. The four observations by Lyman in 1874 give 56″·34, 51″·54, 54″·63, and 51″·49, the mean value, 53″·50 agreeing closely with the result obtained from Mädler's. Combining both series with Lyman's results in 1866, Mr. Neison obtains 54″·65 as the probable horizontal refraction of the atmosphere of Venus; whence we may infer that the surface density of the atmosphere of Venus is not far on either side of 1·892 times that of the earth's.

Imagined Specular Reflexion from Surface of Venus.—Mr. Brett, the landscape painter, who some time since startled the astronomical world by stating that he had seen the solar corona when there was no eclipse and without telescopic aid, has lately made another remarkable discovery, stating in a paper read before the Astronomical Society (but cruelly suppressed by the editor), that he had detected unmistakable specular reflexion from the surface of Venus. Capt. Noble was led to investigate the question. "His first step was to examine the planet with the telescope in the usual way, and no signs of specular reflexion could be seen. He then, using a power of 255, interposed a graduated shade of dark glass. The first part to fade away was, of course, the fainter portion near the termination, then the cusps, and finally the bright part of the limb; not the slightest sign, trace, or indication of specular reflexion being visible."

Photometric Experiments upon Venus.—Mr. J. J. Plummer has made some photometric experiments upon Venus by a new and ingenious method,

based on the fact that the light of Venus is so strong that objects placed in it cast a well-defined shadow. (Mr. Plummer somewhat strangely says that Venus is frequently bright enough to cast a well-defined shadow!) "The plan I have adopted," he says, "has been to compare the light of the planet with that of a standard sperm-candle burning 120 grains of wax per hour, and to vary the distance of this until the shadow it casts upon a screen of white paper has an equal degree of intensity to that given by the planet. It being found impossible to get the two shadows on the same screen, separate screens were arranged for each, and brought as near to each other as possible. The arrangement was therefore a modification of Rumford's photometer. The objects of which the shadows were observed were two equal cylindrical steel wires of $\frac{1}{14}$-inch diameter, placed in a dark room, nine feet in front of their respective screens; but the judgment was further assisted by noting also the shadows of the wooden laths to which the wires were attached, and of which the thickness was about $\frac{1}{2}$ inch. These conditions were preserved throughout the whole of the observations. To protect the candle from wind, it being necessarily placed in the open air, it was fixed within a lantern, which was itself enclosed, except upon one side, in a rough wooden box, painted a dull black. These precautions are believed to have been sufficient for their purpose." The result of these observations is to assign to Venus at her greatest brilliancy almost exactly one-800th part of the light of the full moon. As Bond's observations assign to Jupiter at mean opposition one-6430th of the light of the full moon, and to Venus at her greatest brilliancy 4·864 times the brightness of Jupiter, it follows that Mr. Plummer's estimate of the brightness of Venus exceeds by about 65 per cent. the estimate deducible from Bond's observations (one-1322nd part of the light of the full moon). Mr. Plummer considers that "since the methods employed are entirely dissimilar, and since Bond's investigation has chiefly to do with the moon and Jupiter, both of which Bond observed at altitudes generally much greater than that at which the observation of Venus is possible, this discordance does not prove much." Perhaps not; but, in our judgment, it disproves a good deal.

Proper Motion of Bright Spots on Jupiter.—Mr. Brett has observed that some spots on Jupiter during the late opposition were affected by a considerable proper motion. As these observations required only a good eye for position, reliance can in all probability be placed upon them. Mr. Brett has not corrected his computation for the effects of the planet's retrograde motion; taking that into account, the spots observed would seem to have travelled at a rate of fully 150 miles per hour. This is not quite so great as the proper motion of a rift observed by Baxendell of Manchester, one end of which travelled at a rate of 190 miles per hour for a period of at least six weeks. The spots observed by Mr. Brett were judged by him to be 6,000 miles in diameter, and from the character of their shading and shadows he regarded them as globular in form—an opinion which will probably not be accepted unless stronger evidence can be advanced in its favour.

The Rings of Saturn.—Mr. Trouvelot, of the Harvard Observatory, Cambridge, Mass., has made some interesting observations upon the rings of Saturn. From these it appears that the rings present all those character-

istics which we should expect from their now generally recognised constitution. In particular the dark ring, the phenomena of which have been thought by some to be inconsistent with the theory that the rings are made up of multitudes of small satellites, has presented appearances for which no other theory seems able to account. The inferior portion of the dark ring, where it crosses the disc of the planet, loses itself in the planet's light. This ring again is no longer transparent across its entire width, but is denser near its exterior part, in such sort that from about the middle of its width to its exterior edge it does not permit the edge of the planet's disc to be seen through it. Lastly, the matter composing the dark ring is aggregated here and there in small masses, which almost entirely prevent the light of the planet from reaching the observer.

Duplicity of the Solar Dark Line 1474.—Professor Young has made an important discovery by showing that this now celebrated line is double, the components being separated by about $\frac{1}{7}$th of a division of Angstrom's scale, or by about $\frac{1}{40}$th of the distance between the D lines. Only one of the components belongs to the spectrum of iron, and doubtless the corona line which had been satisfactorily shown to correspond in position with 1474, accords with the other component, not with the component belonging to iron. To what element the corona line really appertains has yet to be determined.

BOTANY AND VEGETABLE PHYSIOLOGY.

The Celtic Race of Pears.—An interesting paper to botanists who are lovers of the antique is given in a late number of the "Journal of Botany" by Dr. Masters. The part of most antiquarian interest is that which refers to the Celtic race, to which the Persian, French, and Devonshire forms belong, and is itself a quotation. It says:—"Dr. Phené visited Brittany, to trace practically any connection—if such could be found—between the legends which connect the 'Isle of Apples' of Arthurian repute with that locality, and those which connect it with Britain. King Arthur, it appears. is supposed to have been buried either in the Island of Avalon (Glastonbury), in England, or in that of Aiguillon in Armorica, the equivalent of Isle of Avalon being Isle of Apples. An island in Loch Awe, in Argyllshire, has a Celtic legend containing the principal features of Arthurian story, but in this case the word is 'berries' instead of apples. These particulars were fully given in a paper read on June 10, 1875, by Dr. Phené before the Royal Historical Society, in which he expressed a belief that the legend of the mystical Arthur was derived from the character of Arjuna given in the Indian poem, "Máhá Bárata." After closely examining the Island in Loch Awe, and Avalon in Somersetshire, he concluded his researches by a visit to Armorica, Brittany. He there observed a tree which helped him to the apples of Avalon and the berries of Loch Awe, for the apples on the tree were berries. The specimen he has submitted to us is the *Pirus cordata* of Desvaux, and it is interesting to note, in support of Dr. Phené's argument, that it has been found in Western France—perhaps in South-western

England, if the plant found by Mr. Briggs near Plymouth, and called by Dr. Boswell-Syme 'Pyrus communis, var. Briggsii,' be the same—and nowhere else in Europe. Both countries had their western shores occupied, anterior to the invasion of the Cymry, by a peculiar race of people having strong Oriental characteristics, and which people some authors describe as occupying the country as far north as Argyllshire—the evidences of such occupation having been laid before the British Association at Bristol in September 1875, in Dr. Phené's paper on that subject—while the same tree is found on Mount Elbruz in North-east Persia—a country not remote from that which formed the arena of Arjuna's exploits, and whence it would seem to have been imported to the west of Europe."

On Floral Æstivation.—The Rev. G. Henslow read a paper on the above subject before the Linnean Society on June 1. Mr. Henslow referred to his previous paper read before the Society, in which he regarded the opposite as the fundamental arrangement of phyllotaxy in Dicotyledons, and described the various modifications of imbricate æstivation. Starting from the ordinary pentastichous or quincuncial mode, in which two leaves of the cycle are external and two internal, whilst one is half outside and half in, special attention was called to the "half-imbricate" and "imbricate proper" methods, in both of which there are one external, one internal, and three intermediate leaves; the "imbricate proper" is converted into the convolute mode, in which all the leaves are intermediate, by the first leaf of the cycle being overlapped by the adjacent third leaf. The "vexillary" and "cochlear" modes, and those of many other irregular flowers such as *Cassia*, are to be referred to the "half-imbricate." The author agreed with Professor A. Gray in distinguishing "convolute" from "contorted." A new theory of the nature of cruciferous flowers, which derived them from a primary type by symmetrical reduction of the parts in each whorl, was explained; and chorisis was objected to as an explanation of the pairs of long stamens. The frequency with which the corolla is found to develope subsequently to the stamens was also mentioned in objection to Pfeffer's view of the corolla of *Primula* being an outgrowth of the androecium.

The Hygroscopic Mechanism by which Seeds bury themselves was some time ago the subject of a paper read before the Linnean Society by Mr. Francis Darwin, the son of the celebrated naturalist, and it is thus reported by the "Journal of Botany" (No. 161):—"The seeds observed were those of several grasses and of *Anemone montana*, but *Stipa pennata* was specially examined. This has a strong awn, the lower part vertical and twisted with two knees, and a long horizontal upper feathered portion. Moisture causes the spiral portion to untwist and the horizontal part to revolve, the knees disappearing and the whole awn becoming straight; drought reverses the process. In nature the flat feathered portion is readily entangled in vegetation, and the seed rests vertically with its point on the soil. When the spiral untwists with moisture, the horizontal part being prevented from revolving, that motion is transferred to the seed, and to this being added pressure on its point it becomes screwed into the ground. With dryness and the reversal of the screw the seed is not drawn out again, but curiously is thrust deeper down by additional mechanism. Heat acts in the same way as moisture. The cause of torsion as explained by Hildebrandt and Hanstein

the author thinks insufficient, and shows that the power resides in the individual cells of the awn, which when isolated behave precisely as the whole awn, with regard to moisture, heat, and dryness."

A Lichen rare in Great Britain.—The occurrence of *Thelocarpon Laureri* in Britain is so rare, that its appearance in great quantity is worth recording. Three habitats are given for it in Leighton's "Lichen-Flora," all in Shropshire; and, says Mr. W. Phillips, writing in the "Journal of Botany," No. 161, "I have now to add a fourth, also in the same county. In the autumn of 1874 a plantation on the Arcoll Hill, an outlier of the Wrekin, by some accident was set on fire, and a large portion was destroyed. The undergrowth, consisting of heather, bilberry, brakefern, &c., was so dry that no efforts were able to arrest the flames till the whole area enclosed by the cart-ways for drawing timber was left bare and black; these formed an effectual barrier and arrested the conflagration. The damage extended over several hundred acres. Last autumn a new growth of vegetation began to make its appearance on the charred surface, amongst which were conspicuous *Marchantia polymorpha*, *Funaria hygrometrica* (la Charbonnière), seedlings of *Pteris aquilina*, and a quantity of fungi, such as *Agaricus carbonarius*, Fr., *Peziza trachycarpa*, Curr., *Rhizina undulata*, Fr. On visiting the place this spring I found on the peaty portions a large quantity of *Thelocarpon Laureri*, in small patches from an inch to a foot across, extending over a very large area. At first sight I mistook it for the early growth of a Lichen-thallus, but when once recognised the eye became accustomed to its peculiar citron colour and scattered mode of growth."

CHEMISTRY.

Errors of the New Catalogue of the Loan Collection.—The "Academy" (August 12) has published a report on this book which has dealt justly, but severely, with the authors, whoever they are. It says:—"While the collection may be justly styled scientific, that term cannot be applied to the method in which the objects are classified in the catalogue, and many of the blunders are of such a character that it is hard to conceive it possible that they could escape the notice of a printer's reader, still less that they should pass unchallenged the scrutiny of a scientific editor." After giving a series of the errata, the article thus concludes:—"In printing the names and addresses of the contributors the same want of accuracy has been shown, and so we read of the 'Physical Institute of the University of Freiberg, Baden.' In many cases the foreign names of pieces of apparatus have not been translated at all, although an English equivalent is to be found without difficulty: we meet, for example, with such words as 'étuve,' 'stativ,' 'bobine,' &c.; others again which have been rendered into English are not in the form familiar to the man of science: such are, 'charcoal sticks' for carbon points, 'effective substance' for active principle, 'Grove pile,' 'chroïtes crystals,' 'rhomboïd of Iceland spar,' &c. The mode of rendering other scientific terms in common use in England is equally unhappy, and of these we may instance 'atterism,' 'apparatuses,' 'a chemical harmonica,' and

'Spee-gear, bottle in case,' whatever that may be; while the term 'pantigraph' is still retained in the introduction. The blunders appear to be impartially distributed over the various sub-sections; while we have 'wolfram phosphide,' 'siliconcalcium,' and 'molibdate of lead,' on the one hand, we meet with 'Kemala' and 'Saborandi' on the other; and among the rocks and minerals, 'Trooshte' for troostite, 'dimyte,' repeated, for dunite, 'Garnet fils,' 'Rosed felsyte,' 'Hokscharowite,' &c. Finally, we will confine ourselves to directing attention to about a dozen of the errors in the names of persons which we have met with during a superficial examination of this revised edition of the Catalogue:—'Andrew' for Andrews, 'Berzélius,' 'Bilstein' for Beilstein, 'Cloës' for Cloez, 'Sir Humphery Davy,' 'Eisenrohr' for Eisenlohr, 'Erltenmeyer' for Erlenmeyer, 'Fritzshe,' 'Kakuli' for Kekulé, 'Rutlerow,' 'Rudnen' for Rudneff, 'Schick' for Schiek, 'Siemgan' for Siemens, 'Smith' for Smithson, and 'Wrohlesky' for Wroblevsky."

Nitrogen and Albumen in the Milk of Women and Cows.—Dr. Leo Liebermann states in Liebig's Annalen (Bd. 181, Heft. 1), that both in Brunner's and in Hoppe-Seyler's method a considerable part of the albuminoids escapes precipitation. Haidler's method, on the other hand, gives the total amount of the lacteal albuminoids, which may also be entirely thrown down by means of tannin. In addition to casein and albumen a third and distinct albuminoid body is present, but there is no nitrogenous body found in milk except such as belong to the albuminoid class.

American Chemical Society.—The "Chemical News" of July 14 says that at a meeting of American chemists, held lately at the New York College of Pharmacy, it was resolved to form a society to be called "The American Chemical Society," and at a subsequent meeting the following officers and committees were appointed:—*President*—John W. Draper. *Vice-Presidents*—J. Lawrence Smith, Frederick A. Genth, E. Hilgard, J. W. Mallet, Charles F. Chandler, Henry Morton. *Corresponding Secretary*—George F. Barker. *Recording Secretary*—Isidor Walz. *Treasurer*—W. M. Habirshaw. *Librarian*—P. Casamajor. *Curators*—Edward Sherer, W. H. Nichols, Frederick Hoffmann. *Committees on Papers and Publications*—Albert R. Leeds, Herrmann Endemann, Elwyn Waller. *Committee on Nominations*—E. P. Eastwick, M. Alsberg, S. St. John, Chas. Fröbel, Chas. M. Stillwell.

The Action of certain Filters has been gone into by Mr. Alfred Wanklyn, who reports as follows in the "Chemical News" (July 14) on the action of the "Silicated Carbon Filters," which, as we have already reported in a former number of this journal, are unquestionably the best species of filter. A solution of hydrochlorate of morphia in common London water was prepared by taking 1·320 grms. of hydrochlorate of morphia, dissolving it in water, and diluting the solution to 10 litres. In this manner a solution containing 0·132 grm. of the hydrochlorate per litre of water was obtained. Submitted to the ammonia process, this solution was found to yield 2·60 m.grms. of albuminoid ammonia per litre. Five litres of this solution were then allowed to run through the same silicated carbon filter which had been employed for the experiments on quinine, described before, and the 5 litres of filtrate were then thrown away. In this manner the most simple displacement of the liquid occupying the pores of the filter was ensured.

About 5 more litres of the solution were next run through the filter, and the filtrate was examined with the following results:—Milligrams of albuminoid ammonia per litre of liquid—No. 1, 0·06; No. 2, 0·04. Showing how completely the filtration had removed the morphia from the solution. As a further corroboration, advantage was taken of the reducing properties possessed by morphia, which decolourised standard solution of permanganate, and which may be titrated with such a solution. Before submitting it to filtration, 100 cubic centimetres of the solution of morphia reduced 8·5 c.c. of decinormal permanganate solution. After filtration, 100 c.c. of the liquid did not reduce any appreciable quantity of the permanganate. Thus it has been proved that one single filtration through a thickness of 6 inches of "silicated carbon" is sufficient to remove morphia from a solution containing 132 m.grms. of the hydrochlorate of morphia in 1 litre of water (or 9·24 grains per gallon).

GEOLOGY AND PALÆONTOLOGY.

The Geology of the Carrara Marbles.—Mr. G. A. Lebour makes the following observations on this subject in the "Geological Magazine" (July). The statuary marbles of Carrara have in turn been referred to the—

1. Eruptive series. 1829, Savi.
2. Cretaceous. 1833, Savi.
3. Oolite (without further specialization). 1843, Savi.
4. Palæozoic, probably Carboniferous. 1845, Coquand.
5. Jurassic and Liassic. 1845, Pilla.
6. Infra-Lias and Rhætic. 1847, Pilla.
7. Lower Lias. 1851, Savi and Meneghini; 1856, Cocchi; 1862, Savi.
8. Base of Verrucano (Trias or Permian). 1862, Capellini.
9. Lower Carboniferous. 1864, Cocchi; 1875, Coquand. Generally admitted.

Let us hope that these ill-treated beds have now found a permanent rest. Still it is painful to see how long it has taken for the truth to prevail in this case. Had not the unlooked-for discovery of fossiliferous carboniferous beds taken place, the very clear stratigraphical evidence adduced by Coquand in 1845, strengthened by his determination of triassic beds at Spezia, would have gone for nought against the preconceived theories of high authority.

The Glaciers of the North Slope of the Alps.—At the meeting of the Geological Society (May 24), reported in the "Geological Magazine" (July), Professor Alphonse Favre, F.M.G.S., read a paper on the above subject. The author illustrated his remarks by a map on a scale of $\frac{1}{250000}$, showing the space occupied by the old Swiss glaciers at the time of their greatest extension, and founded in part upon evidence obtained since 1867, when he, in conjunction with Professor Studer and M. L. Soret, issued an "Appel aux Suisses" for the preservation of erratic blocks. He said that in existing glaciers two parts may be recognised—an upper one, the reservoir or feeding glacier, and a lower one, the flowing glacier. Applying this division to the old glaciers, it appears that in the glaciers of the Rhone and Rhine the

flowing glacier which occupied the plain had a surface nearly equal to that of the feeding glacier which was situated in the mountains. By means of several tables M. Favre showed the height attained by these glaciers, their thickness, the slope of their upper surface, &c., at various points in the Alps, the Jura, and Swabia, and deduced as the result of the comparison of these numbers:—1. That the Rhone glacier passed over several of the chains of the Jura, and that the ice covering these, far from being an obstacle to the extension of the glaciers of the Alps, actually reinforced them, and served them as *relays*, the glaciers of the Jura having carried far on the Alpine erratic blocks. 2. That the slopes of the upper surface were variable, and were null, or nearly so, over considerable spaces.

On the Ice Age in Great Britain.—In a paper published in the "Proceedings of the Edinburgh Geological Society" Mr. Ralph Richardson gives the facts with regard to the shallow depths of ocean between Great Britain and Iceland and Greenland on one side and over the German Ocean on the other, and presents reasons for believing that there was dry land over the region in the glacial era; that the glaciers of Great Britain came over this emerged land from the north and west; and that the cold of the glacial era was due in part at least to the closing thus of the Arctic, and excluding thereby the Gulf Stream. The facts appear to sustain the conclusions. The depth between Britain and Iceland mostly does not exceed 100 fathoms, and nowhere exceeds 1,000; and one tract of sea extending in a straight line from the eastern coast of Greenland *viâ* Iceland and Faroe to Scotland does not exceed 500 fathoms. The depth of the sea in the English Channel is only about 20 fathoms, and the average depth of the North Sea or German Ocean is not over 40 fathoms, or 240 feet. The depth between Britain and Greenland is small compared with the average depth of the Atlantic. The author closes with the conclusion, that one of the oscillations of level, such as have often occurred over the earth's surface, had the effect to "unite Britain and Northern Europe with Greenland and the Arctic regions;" "to give the polar ice-fields access to Europe;" "to divert the course of the Gulf stream and free North-western Europe from its influence; and, in conjunction probably with some diminution in the influence of the sun, to produce a glacial epoch."

Ice and Ice-work in Newfoundland.—In a paper on this subject in the "Geological Magazine," Aug. 1876, Mr. J. Milne gives the following as the conclusion to be drawn from a series of observations on the subject:—"If Newfoundland has been steadily rising during past ages, as it now appears to have done at no very remote geological period, it may have been beneath the surface of the ocean. During the period when it was undergoing elevation, no doubt a considerable amount of débris and boulders were dropped by icebergs over its surface; when the Laurentian backbone, which would be the first land to emerge, reached the surface, it formed a barrier for the coast-ice which would carry its load of boulders and strew them with those of the bergs. This latter might to some degree have been influential in giving a definite character to the rising area. After the final emergence, the climate of Newfoundland might still have been a cold one, and the same highlands which gave birth to the coast-ice, probably next gave birth to glaciers which scooped and hollowed out a great portion of

the remaining marine drift, and left the island with its present contour. After the raising of the great north-east and south-east ranges, first coast-ice flowed east and west, and afterwards the glaciers followed in a similar direction, and thus perhaps the origin of the boulders, those which are so curiously perched being due rather to the latter than to the former. Thus it would seem that icebergs and coast-ice preceded glaciers, but to say what might have come before the former of these agents would only be diving deeper into the depths of a sea of speculation."

The Ash-showers of Iceland.—Professor Nordenskiold, in the "Geological Magazine" (June), has an important paper on this interesting subject. From this we extract the following:—"Our knowledge of these eruptions, however, unfortunately is not as yet founded on any scientific examination; and it is perhaps the less necessary to repeat here the interesting accounts of those grand phenomena that have appeared in the newspapers, as I expect to have an opportunity another year of returning to the subject, since the region will probably be visited next summer by a distinguished geologist, well acquainted with the natural history of Iceland. I will only mention that the eruption began in the month of Dec. 1874, and then continued with shorter or longer intervals from numerous craters situated in the interior of the country, partly on Dyngjufjäll, partly in the northern part of Vatna-jökul, or in the region between these enormous glaciers and the great snow-clad volcano Herdabreid. The most plentiful ash-rain on Iceland itself took place in consequence of an eruption which began at the place last mentioned on March 29, and the ashes which fell in Scandinavia probably belong to the same point of time, in which case less than twenty-four hours was required for carrying the ashes from Iceland to Scandinavia; that is, for their passing over a distance of 200 Swedish miles, or 2,000 kilometres. Geological science has recorded many accounts of the fall of volcanic ashes, where the ashes have been carried by the wind to very remote regions; among others that ashes had already been carried, a couple of centuries ago, from Iceland to Bergen, on the west coast of Norway; but no example of so extensive a spreading of volcanic ashes with the wind, as from Iceland to the east coast of Sweden, is previously known. On Iceland the ashes fell in such quantity that at some places they covered the ground to a depth of 6 inches, and destroyed the pastures. The cloud of ashes was for several hours so close that the sunlight could not penetrate it, and lights required to be kindled in the middle of the day. The ashes must also have fallen in considerable quantity in the sea between Iceland and Norway, and on its bottom there are doubtless found places where the remains of such falls collect during centuries without any considerable mixture of foreign matter. Here must be formed thick beds of volcanic ashes, which in the course of geological ages gradually harden together, and are metamorphosed to rocks of nearly the same composition, and therefore also strongly resembling those which in molten form burst forth from the interior of the earth; and we have here, doubtless, the key to the extension over boundless regions of the earth of stratified so-called volcanic rocks, a circumstance to which I have already long ago drawn attention with reference to the occurrence of plutonic rocks regularly stratified in the polar regions."

American Earthquakes in the First Quarter of the Year.—In a good

summary of recent earthquakes, which appears in the "New York Times" for July, Mr. C. G. Rockwood gives the following summary of the reported earthquakes during about the first three months of the present year (1876):—

Jan. 7.—Three shocks at the Island of St. Thomas, W.I., in the morning, the first at about 4 o'clock, the second at about 4.30, which was very severe, and the last three minutes later.

Jan. 7.—A shock at 2.20 P.M. at Warner and Contoocookville, N.H. Its apparent course was from west to east, and its duration two minutes.

Jan. 8.—A shock at 4.30 P.M. at Lockport, N.Y.

Jan. 15.—A severe shock at midnight at China, Me.

Jan. 21.—A shock between 3 and 4 A.M. at San José, Santa Cruz, and San Francisco, Cal.

Jan. 27.—Two shocks at Adrian, Mich.

Jan. 29.—A shock at 9.05 P.M. at Annapolis, Md.

Feb. 7.—A shock in the city of Mexico.

Feb. 27.—A shock at Detroit, Mich.

March 25.—Two slight shocks at 6 A.M. and 1 P.M. at Oaklands, Cal.

April 10.—A shock was felt in a large portion of St. Mary's County, Md., attended by a rumbling sound.

MEDICAL SCIENCE.

The Form in which Iron exists in the Spleen.—MM. Picard and Malassez, starting from the fact that the proportion of iron in the spleen is greater than can be accounted for by the blood it contains, proceed to enquire whether this iron is present in the form of some special compound, or simply in that of hæmoglobin fixed in the splenic tissue ("Comptes Rendus," April 10, 1876). The latter view was found to be the correct one. The following was the mode of investigation adopted. The gland was thoroughly washed out by allowing a stream of salt solution (shown by Kuhne to be incapable of dissolving hæmoglobin) to flow through it until it escaped colourless from the splenic vein. Notwithstanding the complete removal of the blood, the organ still retained its deep-red colour. A stream of distilled water was then admitted into the artery; it issued bright red from the vein. After about two litres had thus permeated the gland, the latter was found to have lost its red colour, and no longer to tinge the water flowing through it. The colouring matter dissolved by the distilled water was proved by its behaviour towards oxygen, carbonic oxide, and reducing agents, to be ordinary hæmoglobin. The decolourised spleen was then submitted to analysis, and found to be entirely free from iron. Hence the authors conclude that iron exists in the splenic tissue as a constituent of hæmoglobin identical with that of the red corpuscles of the blood.

A Poisonous Dye in Wines.—It has recently been discovered that the aniline dye known as fuchsine, or magenta, is largely employed by dishonest wine-growers in France for improving the colour of claret, and masking its dilution with water. Unpleasant symptoms have been observed to follow

the use of this medicated beverage; and MM. Feltz and Ritter have accordingly made some experiments in order to ascertain how far the added colouring-matter ought to be blamed for them ("Comptes Rendus," Juin 26, 1876). They found that half a gramme of fuchsine in solution, taken on an empty stomach, caused deep redness of the ears, intense itching of the mouth, and slight swelling of the gums. The wine was stained of a deep red colour. When the dose was repeated day after day for a fortnight, diarrhœa and albuminuria were developed in addition to the above symptoms. When fuchsine was injected into the stomach or the veins of a dog, it produced effects similar to those observed in the human subject; when the dose was sufficiently large or frequently repeated, albumen invariably made its appearance in the urine; and this symptom was found to be due to a peculiar degeneration of the cortical substance of the kidneys. See also "Academy," Aug. 12.

Rescuing Drowning Persons.—M. Woilley is reported to have devised an instrument which he calls a *spirophore*, for resuscitating drowned persons and warding off the risk of death by asphyxia in certain diseases ("Comptes Rendus," Juin 19, 1876). It consists essentially of a metal cylinder, closed at its lower end, and large enough to contain the body of a full-grown man. The upper end of the cylinder is closed by an elastic india-rubber diaphragm, with a hole in the middle, through which the head of the patient projects. The interior of the cylinder is then partially exhausted by a sort of air-pump; with each stroke of the piston the chest of the patient expands, his diaphragm sinks, and air rushes into his respiratory passages. One advantage of this method of performing artificial respiration is that the air is never forced inte the lungs under a pressure higher than that of the atmosphere; there is no risk of damage being inflicted on the delicate pulmonary tissues, as sometimes happens when insufflation is resorted to. Experiments on the dead subject showed that the average amount of air introduced at each inspiration was nearly twice as great as that drawn in during ordinary breathing. The main objection to this ingenious contrivance lies on the surface—it is not likely to be at hand when wanted, and cannot therefore compete with methods of artificial respiration which, like those of Marshall Hall and Silvester, require nothing more than a certain degree of skill and readiness on the part of the bystanders.

METALLURGY, MINERALOGY, AND MINING.

Roscoelite a Vanadium Mineral.—In "Silliman's American Journal" (July) Mr. F. A. Genth says that he is indebted to Dr. James Blake, of San Francisco, California, for a small quantity of the very interesting mineral, which he called "Roscoelite," in honour of Professor Roscoe, whose important investigations have put vanadium in its proper place among the elements. Roscoelite occurs in small seams, varying in thickness from $\frac{1}{20}$ to $\frac{1}{10}$ of an inch in a decomposed yellowish, brownish, or greenish rock. These seams are made up of small micaceous scales, sometimes $\frac{1}{4}$ of an inch in length, mostly smaller and frequently arranged in stellate or fan-

shaped groups. They show an eminent basal cleavage. Soft. The specific gravity of the purest scales (showing less than one per cent. of impurities) was found to be 2·938 ; another specimen of less purity gave 2·921. Lustre pearly, inclining to submetallic. Colour dark clove-brown to greenish-brown, sometimes dark brownish-green. Before the blow-pipe it fuses easily to a black glass, colouring the flame slightly pink. With salt of phosphorus gives a skeleton of silicic acid, a dark yellow bead in the oxidising flame, and an emerald-green bead in the reducing flame. Only slightly acted upon by acids, even by boiling concentrated sulphuric acid; but readily decomposed by dilute sulphuric acid, when heated in a sealed tube at a temperature of about 180° C., leaving the silicic acid in the form of white pearly scales, and yielding a deep bluish-green solution. With sodic carbonate it fuses to a white mass.

The Chemical Composition of Durangite. — Mr. G. Brush, whom mineralogical readers will remember described this mineral as long ago as 1869, says that he is again indebted to Mr. Henry G. Hanks, of San Francisco, for a new supply of the crystals obtained in recent explorations. These crystals are much smaller than those previously examined, being from one to three millimeters in diameter, and they are of a darker shade of colour. The former were loose detached crystals, while these are associated with, and in some cases attached to, rolled fragments of crystallised hematite and cassiterite. The density of the small dark-coloured crystals is 4·07, while that of the purest of the bright-coloured crystals before described is 3·937. In all other physical characters there is a perfect correspondence between the two varieties. The chemical examination of the dark-coloured small crystals has been undertaken, at my request, by my assistant, Mr. George W. Hawes, first to estimate the amount of fluorine in the mineral, which in two determinations he found to be 7·67 and 7·49 per cent., and Mr. Hawes as also placed at my disposal for this article a complete analysis of this variety of the mineral. The fluorine was determined directly by Wöhler's method as modified by Fresenius. To determine the arsenic acid and the bases, the mineral was decomposed by sulphuric acid, and the arsenic weighed as sulphide; the alumina, iron, and manganese obtained in the analysis were carefully examined to ascertain their purity. The soda and lithia were weighed as sulphates and then converted into chlorides and separated by ether and alcohol. The results of the analysis are as follows:—

	I.	II.
Arsenic acid	53·11	—
Alumina	17·19	—
Ferric oxide	9·23	—
Manganic oxide.	2·08	—
Soda	13·06	—
Lithia	0·65	—
Fluorine	7·67	7·49
	102·99	

Silliman's American Journal, June 1876.

MICROSCOPY.

Mr. Sorby on Count Castracane's views on ultimate Vision with the Microscope.—The President of the Microscopical Society (Mr. W. C. Sorby) gave a valuable address this year, in which he referred to and elucidated Helmholtz's views. This address was subsequently read by Count Castracane, who made a series of observations, showing that vision extended further than Helmholtz's views would allow. These are now commented on as follows by Mr. Sorby:—"With reference to the facts here described by Count Castracane, I wish to offer a few remarks. It appears to me that the visibility of the fine lines of Nobert's test-plates depends on several different circumstances. The light must be thrown in such a manner as to be definitely intercepted by the marking on the glass, or they could not possibly be seen; and we have then to consider the effect of interference fringes, as well as the quality of the microscope itself. I do not see that there ought to be any serious difficulty in explaining, on Helmholtz's principles, the resolution of Nobert's nineteenth band. With such an illumination as that adopted by Count Castracane, it appears to me very probable that the interference fringes would so far coincide with the true lines as not to prevent a satisfactory definition. At the same time I am anxious to make it fully understood that in my address I endeavoured more to point out the results that would follow from Helmholtz's theory, than to examine whether it is or is not in every respect true. I should be one of the last to wish it to be looked upon as a final solution of the problem. I think many questions remain to be cleared up by the actual observations of persons conversant with the theory, and accustomed to the practical use of high powers. I am also inclined to believe that several crucial tests ought to be examined. Amongst these I would especially suggest the study of fine lines at very close yet *unequal* intervals, and of lines at equal intervals with one or two *missed out* here and there. Theory indicates that such tests would be far more difficult to see correctly than lines ruled at regular and equal intervals; and an examination of such tests ought to afford much information respecting both the final powers of our microscopes and the physical constitution of light itself. Helmholtz, of course, assumes the truth of the undulatory theory; and though in the highest degree probable, it would perhaps be premature to conclude that it is absolutely certain, when applied to the explanation of every phenomenon, especially in such a case as that under consideration."

Diatomaceæ Absorbed in their Entire State by the Roots of Plants.—Some very curious observations have been made by Professor P. B. Wilson, of Baltimore, U.S.A., which seem to show that the Diatomaceæ, when applied to the earth in which corn was grown, absolutely passed in their entire condition through the roots, and were found in the stems of the corn. In "Silliman's American Journal" (quoted by the "Monthly Microscopical Journal," August 1876) Professor Wilson says:—"To demonstrate this theory, my friend G. I. Popplein, Esq., of this city, suggested the application of infusorial earth of the Richmond formation—found in large quantities on the western shore of the Chesapeake Bay—to land sown in wheat. I

have obtained straw from wheat so grown, and have found, after it has been treated with nitric acid, and the silicious remains placed on the field of the microscope, that it consisted wholly of the silicious shields of Diatomaceæ, the same as found in the infusorial earth, excepting that the larger discs in their perfect form were absent (*Actinocyclus Ehrenbergii* and *Actinoptychus undulatus*). My conclusions are that they—and there probably may be other forms—are too large to enter the root capillaries. During the coming summer I will attempt, if possible, to make micrometer measurements of both. The discovery of Diatomaceæ in their original form in this wheat-straw precludes the possibility of the infusorial earth having undergone any chemical change in the soil, either by forming chemical combination with the alkalies or the earths, or by suffering physical disintegration from any catalytic action of any salts present in the soil. In the particles of silica placed upon the glass slide, when they were completely separated from each other, the outlines of the individual diatoms were sharply and distinctly defined. On the other hand, when the physical action of ebullition with nitric acid was not sufficient for the complete separation of the particles of the epidermal shield, there was observed a marvellous interlacing of the various forms, showing that they were conveyed by the sap-cells directly to the section of the plant where they were destined to complete its structure. I have examined several specimens of straw, taken at random in the market; the silica in each specimen consisted of plates, very thin, and truncated at the corners."

Musical Sand Examined beneath the Microscope.—A paper on this subject, which is really a somewhat curious one, is published in the last number of the "Proceedings of the California Academy of Sciences" (vol. v). He states that "in order to ascertain, if possible, the cause of the sound that is produced by the sand from Kauai, presented to the Academy at a former meeting, I investigated its structure under the microscope, and I think the facts I have ascertained fully explain the manner in which the sound is produced. As the grains of sand, although small, are quite opaque, it was necessary to prepare them so that they should be sufficiently transparent to render their structure visible. This was effected by fastening them to a glass slide and grinding them down until one flat surface was obtained. This surface was then attached to another slide, and the original slide being removed, the sand was again ground down until sufficiently transparent. The grains were found to be chiefly composed of small portions of coral and apparently calcareous sponges, and presented under the microscope a most interesting object. They were all more or less perforated with small holes, in some instances forming tubes, but mostly terminating in blind cavities, which were frequently enlarged in the interior of the grains, communicating with the surface by a small opening. A few Foraminiferæ were also met with, and two or three specimens of what appeared to be a minute bivalve shell. Besides these elements, evidently derived from living beings, the sand contained small black particles, which the microscope showed to be formed principally of crystals of augite, nepheline, and magnetic oxide of iron, imbedded in a glassy matrix. These were undoubtedly volcanic sands. The structure of these grains fully, I think, explains the reason why sound is emitted when they are set in motion. The friction against each other causes vibrations in their substance, and consequently in the sides of the cavities

they contain; and these vibrations being communicated to the air in the cavities, under the most favourable conditions for producing sound, the result is the loud noise which is caused when any large mass of sand is set in motion. We have, in fact, millions upon millions of resonant cavities, each giving out sound which may well swell up to resemble a peal of thunder, with which it has been compared; and the comparison—I know from others who have heard it—is not exaggerated. The effect of rain in preventing the sound is owing to the cavities in the sand becoming filled with water, and thus rendered incapable of originating vibrations."

PHYSICS.

A New Form of Mountain Barometer has been described by Mr. Emmons in "Appalachia," June 1876, the first number of a new American journal devoted to mountaineering pursuits. In the ordinary mountain barometer we must carry about with us a tube of nearly a yard in length, but Macneill's instrument need not be more than half this length. The tube is open at both ends, and the lower end passes by an air-tight connection through the top of a cylinder, which opens only in a tube below, connecting it with the mercury cistern. The bottom of this cistern is of soft leather, resting upon the end of a vertical screw, as in the Fortin barometer, and the whole cistern, with its mercury, may be separated from the tube and carried independently, the liquid being retained by a stop-cock. To use the instrument, it is hung vertically, and the screw is turned up, forcing the mercury into the cylinder above. Then the liquid rises to the level of the bottom of the open tube, the air above it is confined in the cylinder, and is then under the atmospheric pressure. Now, if we continue to force up the mercury we compress the air in the cylinder, and the liquid rises to a corresponding height in the tube. This compression is continued in all measurements, until the mercury in the cistern rises to a certain fixed point. The corresponding height of the mercury column in the tube, as read upon its scale, will be greater or less, according as we are reading at the sea level or upon a mountain, and if the scale has been previously graduated by comparison with an ordinary mercurial barometer at different pressures, the atmospheric pressure may be read directly from it. The error in reading this instrument is greater than in the Fortin or other barometers, and it is also liable to error from some other sources; but it is an extremely convenient form for transportation, since the mercury may be carried separately from the tube, and the whole instrument is hardly more than half the length of the shortest syphon barometer of the same range.

New Experiments with the Radiometer have been conducted by M. A. Ledieu, and are reported as follows in the "Comptes Rendus" (June 12) and "Chemical News" of July 14:—The radiometer was found to continue revolving when submitted exclusively to a pencil of luminous rays falling parallel to its axis. The author, however, does not draw the conclusion to which a superficial and systematic examination of this result might seem to lead. The experiment performed by M. Salleron at the suggestion of the

author, condemns decidedly the doctrine of emission as an explanation of the movement of the radiometer.

Experiments with Frozen Dynamite.—Some interesting experiments were recently made at the works of the British Dynamite Company, at Stevenston, Ayrshire, with the view of proving that dynamite in a frozen state is as safe to handle and to transport as in an unfrozen state. They are reported in the "Chemical News," July 14. Professors James Thomson and Bottomley, of the University of Glasgow, were present. In the first experiment, several cartridges in a frozen state, and in some parts beginning to thaw, were thrown one by one from the hand, with great force, against an iron plate, without explosion. In the second experiment, a block of iron, about 400 lbs. weight, was allowed to fall from a height of about 20 feet on a light wooden box containing 20 lbs. of dynamite cartridges in a frozen state, and with slight signs of incipient thawing in spots more exposed to the warmth of the air. The box was smashed, and the cartridges were crushed flat and pounded together, but there was no explosion. The crushed cartridges were next made up into two heaps to be exploded. The ordinary detonator shatters, but does not explode the frozen dynamite. The explosion was therefore effected by inserting in each heap a small unfrozen cartridge, with the ordinary detonator inserted into it, and then firing this off by a Beckford fuse. The two heaps were exploded successively, and it is worthy of remark that the explosion of the first, though very violent, did not set the other off.

Sound Attractions.—A recent number of "Silliman's American Journal"—which, by the way, is remarkable for its admirable summary of Physical Science—gives the following account of M. Doorak's recent inquiries. It says:—"M. Doorak has examined the attractions and repulsions of small pendulums hung near sonorous bodies. A square of paper or a piece of cork is hung by a silken thread, and held near a wooden rod, vibrating slowly. Varying the positions of the pendulum, it is sometimes attracted and sometimes repelled. These motions seem to be due to the air-currents approaching or receding from the rod, and the motions of the cork served to determine approximately the directions. These results were verified by the motions of a flame and the indications of a very sensitive water manometer. The air thrust aside by the vibrating rod escapes laterally, repelling light bodies. This is replaced by air forming counter-currents toward the rod, producing the effect of attraction. When the amplitude of the vibrations is small, the rod acts like the prongs of a tuning-fork, and attraction takes place in every direction. In front of the opening of a tube of Kundt, is placed a second open tube, giving the same sound as the first, and suspended by two threads. Making the first tube resound loudly, the second tube is strongly repelled. The same effect is obtained if the second tube gives one of the harmonies of the first. Placing two tubes facing each other opposite the tube of Kundt and perpendicular to its axis, they tend to approach each other. With a very sensitive manometer it appears that in a column of air in a state of permanent vibration, the air at the nodes has an excess of pressure. This accounts for the heaping up of water in the loops of a tube of Kundt. It is explained by admitting that the amplitude of the vibrations cannot be neglected compared with their length. It follows that there ought to be a

continuous motion of the air from a node to a loop. This might be proved by filling the resonant box of a tuning fork with the fumes of chloride of ammonium, and seeing if they are thrown out when the fork is set in vibration. If a bell is filled with water, and a drop of oil allowed to fall on it, the circular film becomes quadrangular when the bell is sounded. The water-currents start from the nodes, and accumulate at the loops. A disk of glass is attached to the end of a rod vibrating longitudinally. If a glass drop is hung opposite the disk, it will be repelled at the centre and attracted around the periphery. There are then, as with air, currents outward at the centre, and counter-currents inward along the edges.

Temperature of the Interior of the Earth.—From observations made on the well of Sperenburg, near Berlin, M. Mohr ("Les Mondes," May 4) concludes that at the depth of 5,170 feet the increment of heat must be nil. A similar decrease of the increment of heat has been observed in the Artesian well of Grenelle. Hence M. Mohr draws conclusions unfavourable to the Plutonian theory.

ZOOLOGY AND COMPARATIVE ANATOMY.

Impregnation of the Boa-Constrictor.—In a recent number of the "American Naturalist," Mr. S. Lockwood makes some interesting observations on the eggs of the above animal—in fact, he puts a very important question to the physiologist. He says: "My friend Dr. Kunzé has shown me an infertile egg of a boa which he lately obtained at the Central Park menagerie. The boa laid twenty-one eggs, each about the size of a hen's egg. The animal made the deposit in sight of her keeper and others. She laid two fertile eggs, and then a sterile one, in regular succession; each third egg was sterile. The fertile eggs had each a young boa within. One came out of its shell immediately after being laid, but soon died. All the others died within their shells. The sterile eggs were albuminous throughout, and cut like cheese and smelled like sperm-oil. Could this be the balance of an impregnation received the year before?

The Cat as a Substitute for the Carrier Pigeon.—It seems that the Belgians have formed a society for the mental and moral improvement of cats. Their first effort has been to train the cat to do the work now done by carrier pigeons. The most astute and accomplished scientific person would have his ideas of locality totally confused by being tied up in a meal-bag, carried twenty miles from home, and let out in a strange neighbourhood in the middle of the night. This experiment has, however, been repeatedly tried upon cats of only average abilities, and the invariable result has been that the deported animal has re-appeared at his native kitchen-door the next morning, and calmly ignored the whole affair. This wonderful skill in travelling through unfamiliar regions, without a guide-book or a compass, has suggested the possibility of cats being used as special messengers. Recently thirty-seven cats residing in the city of Liege were taken in bags a long distance into the country. The animals were liberated at two o'clock in the afternoon. At 6·48 the same afternoon one of them reached his home-

His feline companions arrived at Liege somewhat later, but it is understood that within twenty-four hours every one had reached his home. It is proposed to establish, at an early day, a regular system of cat communication between Liege and the neighbouring villages.

A Cosmopolitan Butterfly.—Mr. S. Scudder gives the following sketch of the distribution of *Vanessa cardui* in the "American Naturalist" for July:—"There is but one butterfly whose range is so extended as to merit the name of cosmopolitan; it is the Painted Lady, or *Vanessa cardui.* With the exception of the Arctic regions and South America, it is distributed over the entire extent of every continent. Australia and New Zealand produce a race peculiar to themselves, while the other large islands south of Asia possess the normal type, which is also found upon small islands lying off the western borders of the Old World, the Azores, Canaries, Madeira, and St. Helena. On the other hand, it has not been discovered upon the small islands off the American coast, such as Guadaloupe, the Revillagegidos, and Galapagos on the western side, or the Bahamas and Bermudas on the eastern; neither does it occur in any of the Antilles, excepting Cuba, and there but rarely. It is reported, however, from islands lying in the middle of the Pacific Ocean, such as the Hawaiian group and Tahiti, but its actual occurrence there is at least doubtful. On the American continent, its southern boundaries will probably be found in Venezuela, New Granada, and Ecuador, but it is abundant even as far south as the highlands of Guatemala, and thence stretches northward over the entire breadth of the continent to the Arctic regions; on the eastern coast it has been found as far as Labrador, and on the west to the eastern shores of Behring's Straits. In the heart of the continent I have taken it upon the Saskatchewan, and Doubleday reports it from Martin's Falls; but Mr. W. H. Edwards does not recollect seeing it in the few collections he has examined from points farther north. As we see it flourishing in the colder regions of Europe and North America, so also is it found on all mountain heights; and Mr. H. W. Bates, writing of the whole genus, distinctly says it is 'found only in elevated places in the neighbourhood of the Equator.' The stations in Southern Asia from which *V. cardui* has been reported—Cashmere, Nepaul, Bootan, and Sikkim—all lie on the flanks of the Himalayas, and the Nilgherry Hills are the highest elevations of the Indian peninsula. In the Alps of Europe this insect flies to the snow level; but in North America, although it may be regarded as one of the commonest butterflies in the elevated central district, it is most abundant at a level of 7,000 or 8,000 feet. Lieut. W. L. Carpenter and others have never found it above the timber line; but Dr. A. S. Packard, jun., has taken it on Arapahoe Peak between 11,000 and 12,000 feet, and on Pike's Peak from 8,000 to within 500 or 1,000 feet from the summit."

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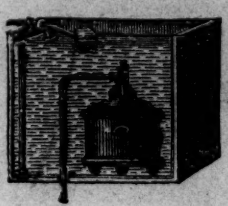

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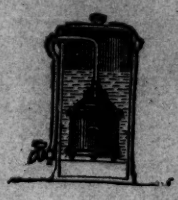

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